humanity's environment but will change humanity itself. The potentialities of biology are different and in some ways profounder than those of physical science, but they are attained less quickly. In the early period when the physical sciences are growing more quickly, men of genius are attracted to them. The power of choosing a suitable activity is one of the qualities of genius. Further, when a branch of human activity has achieved a large development, many sorts of mind find a scope in its size and variety that they could not have found at an earlier stage.

In this study evidence is provided for the view that the French Revolution had a profound influence on the careers of Davy and Faraday. It is argued that Davy's chief greatness was in his work as an expositor of the significance of science and as the voice of the industrialists who desired to apply science to the production of wealth. In the American phrase, Davy sold science to the industrialists.

The career of William Thomson, Lord Kelvin, is seen as the consummation of the process so brilliantly advocated by Davy, the introduction of the scientist into industrial affairs. The chief social significance of Maxwell and Thomson is found in their introduction of the general study of physics into the universities, so that the institutions of advanced learning became adapted to the ends of the industrialists.

An examination of the works of the subjects of this book shows that Faraday's reputation as the greatest of all experimental physicists remains as it is; that Davy's researches were more brilliant than is commonly accepted to-day; that Thomson was a greater personality than scientist, and his importance lay more in technical than pure science, and that his philosophical ideas have proved unfruitful; that Joule was the most extraordinary and peculiar genius of his time; and that Maxwell was a far greater scientist than even his most sympathetic contemporaries believed. It is hoped that this study will show that these men were even greater than is commonly supposed, and that individual scientific

geniuses have less effect on the development of science, in proportion to the contributions of the general body of scientists, than is usually believed.

Four of the five were childless, and the one successful parent was the most unexpected: Joule. These facts confirm the suggestion that scientific genius is often associated with peculiarities of sexual physiology and psychology.

All were earnestly religious. Faraday and Maxwell were exceptionally religious. These facts are probably of important psychological significance. The relations between the characteristics of the scientific and religious ideas of scientists should prove interesting material for psychological study.

This book is not intended to be a learned work. It is the result of an examination of the researches and life of nineteenth-century scientists by a scientific journalist in contact with scientific development in the twentieth century. It is an impression by the past on one close to the present.

I have pleasure in thanking the following for permission to reproduce illustrations: the Cambridge University Press; Messrs. Chapman & Hall, Ltd.; Mr. W. E. Gray; Sir Robert Hadfield; Mr. D. T. King; Sir Joseph Larmor; Messrs. Macmillan & Co., Ltd.; the Physical Society; the Royal Society; and the Master and Fellows of Trinity College, Cambridge.

The bibliographies at the ends of the chapters contain the authorities I have chiefly followed, and I am glad to record here a general acknowledgment of my indebtedness to their writings.

While the book was in the press Professor E. N. da C. Andrade published an interesting comment on the experiment by which Davy claimed to have established the dynamical theory of heat. He doubts whether Davy could have overcome the technical difficulties of producing warm water by rubbing two pieces of ice together, and thinks the correct result must have been obtained accidentally. There appears to be no record of anyone having repeated the experiment since Davy described it nearly one hundred and fifty years ago.

J. G. C.

I

HUMPHRY DAVY

1778–1829

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1778–1829

IN his *Discourse Introductory to a Course of Lectures on Chemistry* delivered in the Royal Institution in 1802, when he was twenty-three years old, Davy said:

"The unequal division of property and of labour, the difference of rank and condition amongst mankind, are the sources of power in civilized life, its moving causes, and even its very soul."

The twentieth-century sociologist, familiar with Karl Marx's conception of social development as a process motivated by the struggles of social classes for power, will perceive at once that the author of this quotation must have been an extraordinary man. Davy was enormously famous during his life. He was a great scientist, and his important discoveries must have given him fame under almost any circumstances, but the fame he achieved was even greater than the magnitude of his discoveries would usually have conferred. The distinguished Thomas Young wrote that his experimental discoveries were "more splendidly successful than any which have ever before illustrated the physical sciences in any of their departments," not even excepting Newton's in optics. How could contemporaries of Davy exaggerate his scientific importance? Several contemporary scientists were his equals as scientists, for instance, Gay-Lussac, Berzelius, Wollaston and Dalton. The explanations of his larger fame are non-scientific. Davy was more important sociologically than any of his scientific contemporaries. He was the chief prophet of the new class of applied scientists. Great though he was

as a scientist, he was even greater as the protagonist of the use of scientific method for the development of civilization. His inspired voice led the nineteenth century pursuit of the application of science to industry, agriculture and medicine. Davy regarded chemistry as a sublime subject. He started his *Introductory Discourse* by a definition of chemistry as "that part of natural philosophy which relates to those intimate actions of bodies upon each other, by which their appearances are altered, and their individuality destroyed." The phenomena of combustion, of the solutions of substances in water, of fire, of the production of rain, hail and snow, and "the conversion of dead matter by vegetable organs" are examples of chemical subjects. Mechanics is dependent on chemistry because the movements of material bodies depend on the qualities of the materials. For instance, the experimental theory of the collisions of material bodies cannot be established from the behaviour of bodies made of materials that interact and decompose each other on contact. Natural history is intimately connected with chemistry because it "treats of the general external properties of bodies" while chemistry "unfolds their internal constitution and ascertains their intimate nature." Natural history examines the "permanent and unchanging forms" of things, whereas chemistry "by studying them in the laws of their alterations, develops and explains their active powers." Mineralogy "consisted merely of a collection of terms badly arranged" until the application of the methods of chemical analysis provided a methodical classification based on the composition of minerals. Even botany and zoology are related to chemistry: "how dependent in fact upon chemical processes are the nourishment and growth of organized beings ; their various alterations of form, their constant production of new substances, and finally their death and decomposition." Medicine and psychology "will be found to have derived from chemistry most of their practical applications." The art of preparing remedies is chemical, and ignorance of scientific principles in the processes of pharmacy has often produced dangerous consequences. "Knowing very little of the laws of his own

existence, man has nevertheless derived some useful information from researches concerning the nature of respiration." "The progress of the astronomer has been in some measure commensurate with that of the chemical artist, through the chemist's perfection of the materials used for the astronomical apparatus." There are no definite lines of distinction between the sciences: "the man of true genius who studies science in consequence of its application . . . will rather pursue the plans of his own mind than be limited by the artificial divisions of language." The value of chemistry is not restricted to its help to other sciences. "It applies to most of the processes and operations of common life." Agriculture is intimately connected with chemical science. "For though the common soil of the earth will produce vegetable food, yet it can only be made to produce it in the greatest quantity, and of the best quality, in consequence of the adoption of methods of cultivation dependent upon scientific principles. The knowledge of the composition of soils, of the food of vegetables, of the modes in which their products must be treated, so as to become fit for the nourishment of animals, is essential to the cultivation of land; and his exertions are profitable and useful to society, in proportion as he is more of a chemical philosopher." "The working of metals is a branch of technical chemistry, and it would be a sublime though difficult task to ascertain the effects of this art upon the progress of the human mind. It has afforded to man the powers of defence against savage animals, it has enabled him to cultivate the ground, to build houses, cities and ships, and to model much of the surface of the earth after his own imaginations of beauty. It has furnished instruments connected not only with his sublime enjoyments, but likewise with his crimes and his miseries; it has enabled him to oppress and destroy, to conquer and protect." "The arts of bleaching and dyeing, which the habits and fashions of society have made important are purely chemical." Tanning is a chemical process of great social importance, which has been improved by chemical research; "and if the improvements resulting from new investigations have not been uniformly adopted by

manufacturers, it appears to be owing rather to the difficulty occurring in inducing workmen to form new habits, to a want of certain explanations of the minutiæ of the operations, and perhaps in some measure to the common prejudice against novelties, than to any defect in the general theory of the art as laid down by chemical philosophers, and demonstrated by their experiments." Davy finds porcelain and glass-making one of the most important of chemical applications. These arts have provided "those elegant vessels and utensils which have contributed to the health and delicacy of civilized nations." They have provided much of the apparatus used in scientific research. The gases and their properties could not have been discovered without them, and "the sublime researches of the moderns concerning heat and light would have been wholly lost to us." The effects of chemical philosophy on the human mind must be estimated from an examination of history. The uncultivated savage is "unable to discover causes, he is either harassed by superstitious dreams, or quietly and passively submissive to the mercy of nature and the elements." Science has given to the "being of civilization" an acquaintance with the different relations of the parts of the external world, "and more than that, it has bestowed upon him powers which may be almost called creative; which have enabled him to modify and change the beings surrounding him, and by his experiments to interrogate nature with power, not simply as a scholar, passive and seeking only to understand her operations, but rather as a master, active with his own instruments.* But, though improved and instructed by the sciences, we must not rest contented with what has been done; it is necessary that we should likewise do. . . . Science has done much for man, but it is capable of doing still more . . . the benefits that it has conferred ought to excite our hopes of its capability of conferring new benefits; and in considering the progressiveness of our nature, we may reasonably look forward to a state of greater cultivation and happiness than that we at present

* Compare Marx's "Hitherto philosophers have sought to understand the world; henceforth they must seek to change it."

enjoy." There is every reason to believe that the general laws of nature may be discovered. "The future is composed merely of images of the past . . . our hopes are founded on our experience" so the degree and strength of the latest operations of the human mind must be ascertained. "The human mind has been lately active and powerful; but there is very little reason for believing that the period of its greatest strength is passed; or even that it has attained its adult state. We find in all its exertions not only the health and vigour, but likewise the awkwardness of youth." The invention of printing has diffused culture and made its survival certain; "the germs of improvement are sown in minds even where they are not perceived and sooner or later the spring-time of their growth must arrive." An improved state of society seems to be approaching fast, as the scientist and manufacturer are daily becoming more assimilated. "The increase of projectors (company promoters) even to too great an extent, demonstrates the enthusiasm of the public mind in its search after improvement." The rich are becoming more interested in science and more attentive to the realities of life. They are giving up many of their unnecessary enjoyments and becoming the friends and protectors of the labouring classes. These developments raise the expectation that "the great whole of society should be ultimately connected together by means of knowledge and the useful arts." This view is not derived from "brilliant, though delusive dreams concerning the infinite improvability of man. We consider only a state of human progression arising out of its present condition. We look for a time that we may reasonably expect, for a bright day of which we already behold the dawn." Besides improving civilization, the pursuit of science gives happiness and pleasure to the individual. The phenomena of the external world are perpetually changing, and compel the observer continually to alter his modes of thinking, so that his attention is held. "The appearances of the greater number of natural objects are originally delightful to us, and they become more so when the laws by which they are governed are known . . . the study of

nature, therefore, in her various operations must be always more or less connected with the love of the beautiful and sublime." In great cities where there are few natural beauties, chemical and physical phenomena are a source of enjoyment to the student, which is unconnected with the labour or misery of others. The business or working man may find in science a relaxation free from the immediate motives of gain or existence. The fashionable may revert to science when they are temporarily bored by the common habits and passions of the world. Even writers, politicians and moralists will find that an acquaintance with the science that represents the operations of nature cannot be wholly useless. "It must strengthen their habits of minute discrimination; and by obliging them to use a language representing simple facts, may tend to destroy the influence of terms connected only with feeling."

Such are the main points of Davy's chemical manifesto. Its animation is a direct expression of his personality. J. G. Lockhart said that Davy's eyes were the finest and brightest he had ever seen. In 1811, nine years after the period of the *Introductory Discourse*, G. Ticknor wrote: "He is now about thirty-three, but with all the freshness and bloom of five-and-twenty, and one of the handsomest men I have seen in England. He has a great deal of vivacity, talks rapidly, though with great precision, and is so much interested in conversation that his excitement amounts to nervous impatience and keeps him in constant motion."

His brother John writes that in stature he was rather small, about five feet seven inches high. His hands, feet and bones were small, but his muscles were large, especially in the feet and legs. He was a good walker, and in the habit of saying his legs were abler than his arms and shoulders. His respiration was unusually rapid, about twenty-six to the minute, though in later life it became slower than usual. This interesting fact and other relevant characteristics will be discussed later in this chapter. His neck was long and slender, his head was small, smooth and rounded. His face was small, but not apparently, owing to an ample forehead that rose very beautifully in a gentle

HUMPHRY DAVY
Aged 23

(Engraved by C. Turner, after the painting by H. Howard)

arch. His features were not perfectly regular. He had an aquiline nose broad at the base, a rather large mouth with prominent underlip, and small irregular teeth. His eyes were light hazel, and his very fine curly and glossy hair was light brown. He had a fair complexion, with much colour. His countenance was very expressive, and responsive to his feelings, and when he was happy he smiled beautifully. His eyes were wonderfully bright, and seemed almost to emit a soft light when animated. He had a full-toned impressive voice that was not musical and which he had cultivated for lecturing. It was very effective on the mixed audiences at general lectures.

After acquiring an impression of the ideas and personality of Davy during the most important part of his career, his early history may be related. He was born in Penzance on December 17th, 1778. His ancestor had come from Norfolk in the time of Elizabeth to act as the steward of the Duke of Bolton's estates. Ever since his family had lived fairly comfortably. They had usually learned to read and write, and never passed into the poor labouring classes. His grandfather was a successful builder, and his father, Robert, was a wood-carver, with a small farm. Robert Davy appears to have been a capable carver. In his youth he had been sent to London to serve his apprenticeship. He was not very energetic, and an unenterprising business man. He died at the age of forty-eight, and left little wealth to his widow and five children.

Davy's origin in the lower middle class is of considerable importance. Later in life he became notoriously snobbish. The lower middle class is the most snobbish of all classes because its members, while possessing the social aspirations of their class, are the most insecure and liable to depression into the labouring class. The desire to remain in the middle class is one of its members' most powerful complexes. It is an important source of the energy of Fascist social movements. Davy's mother was a capable and attractive woman. When left with the prospect of poverty she started a milliner's shop in partnership with a French woman had fled to England during the Revolution. Davy

easily have acquired his admiration for rank and dislike of revolutionaries and Jacobins from the bourgeois virtues of his mother besides the opinions of her partner. It is unnecessary to ascribe his love of patricians entirely to weakness of character. From the quotation on the first page of this chapter it is learned that Davy had a class-theory of social development. In fact, this theory was probably an intellectual expression of his strong-minded mother's social conduct and aspirations. He believed that certain ranks in society, in particular the landed and financial aristocracy, were the creative leaders of civilization. He had a certain intellectual justification in his theory of social progress for his desire to join the patricians.

Paris writes that Davy had a "natural timidity of character which he sought to conquer by efforts that betrayed him into awkwardness of manner, and combine with it an irritability of temperament which occasionally called up expressions of ill-humour." The possession of this temperament and the attitudes learnt in his childhood caused much trouble when he became the President of the Royal Society. He "sighed for patrician distinction in the chair of Newton," and prompted a distinguished colleague to comment: "Sir, we require not an Achilles to fight our battles but an Agamemnon to command the Greeks." Davy's claim of the homage given patrician distinction was indignantly denied by the governing class, though his eminence as a scientist was allowed in the highest degree.

Davy's lively temperament when a child made him a family favourite. He was probably spoiled by his aunts. At the age of five he could read very rapidly, and remained a notably swift reader afterwards. At eight he was deeply interested in ghost stories and superstitions, of which his grandmother had a large store. He never lost superstitious practices. For example, when he travelled in Europe at the height of fame he astonished friends by insisting that they should cross their knives and forks after finishing a course. He was sent to Penzance Grammar School for nine years. The master, Coryton, was incompetent and he learnt little. Then he went to Truro Grammar School,

whose master, Cardew, was one of the most competent of the day. There, also, he made no profound impression. Years afterwards, when advising his mother concerning the education of his younger brother, he wrote: "After all, the way in which we are taught Latin and Greek does not much influence the important structure of our minds. I consider it fortunate that I was left much to myself when a child, and put upon no particular plan of study, and that I enjoyed much idleness at Mr. Coryton's school. I perhaps owe to these circumstances the little talents that I have and their peculiar application."

Davy's idle childhood allowed him to develop his taste for fishing and life in the fresh air. Penzance is peculiarly attractive to the child. There is an unusual variety of landscape. St. Michael's Mount stands in the sea about three miles to the east. The bay is protected from the west winds, and the beach is safe. Most of the roads and paths from Penzance soon end in a sea cove. The aspects of the place are various and yet limited, so that a child's mind is stimulated by a variety of objects which are not forbiddingly vast. The contemplation of excessively large, complicated, or profound objects, such as mountains, cities, or Shakespeare, often inhibits the young mind. Cornwall offers a natural kindergarten landscape. In this environment Davy roamed without restraint. He was an active playmate, and knew the townspeople and the local miners. He used to collect his friends before his home, which existed on the site of the Star Hotel, which is opposite to the present Market House, and where his statue now stands. Under these conditions he grew into a bright but awkward lad, narrow-chested and round-shouldered, with a discordant voice. Afterwards, like Demosthenes, he trained his voice into a powerful mechanism of expression, but when he lectured he spoke with a peculiar accent which sometimes caused him to be accused of affectation.

Davy's mother was the adopted daughter of a worthy local surgeon, John Tonkin. When her husband's affairs became difficult, Tonkin supported Humphry, who went to live in his house when he was nine. After the death of

Robert Davy in 1794 Tonkin advised that Humphry should be apprenticed to a Penzance apothecary, Bingham Borlase. The indenture was dated February 10th, 1795. Tonkin desired Davy to become a general practitioner in Penzance, but Davy himself looked forward to graduation at Edinburgh. Here is an adequate explanation of Davy's early interest in chemistry. Sheele and many other distinguished predecessors of Davy were introduced to chemistry by the same way. However, there is some evidence that his intellectual interests were stimulated in the year preceding the indenture. He left Truro Grammar School in December, 1793, just before he was fifteen. For about a year he was rather idle, and is reported to have indulged in occasional dissipation. He said afterwards that it was a dangerous period of his life. His biographers do not say what the dangerous dissipations of a youth of fifteen might have been. During this period he began to receive French lessons from a M. Dugart, a refugee priest from La Vendée. Paris says the French lessons started during his apprenticeship. The records do not clearly indicate whether Davy accomplished the intellectual orientation of his life before his apprenticeship. His early knowledge of the French language is an important fact. He obtained two chemistry books when he was a youth, one of which was Lavoisier's great *Traité Elémentaire de Chimie*. He may have had a copy of Kerr's translation, but not necessarily. The French Revolution had a powerful influence on Davy, for among other effects it had sent refugees to Penzance who taught him French. Indeed, one of his boyish loves was a French girl, to whom, it is said, he wrote many sonnets. His French acquaintance exposed him to the atmosphere of counter-revolutionaries. His father's death probably made him a more serious student. In 1795, at the age of sixteen, he started to keep note-books, some of which are extant. He made a plan of study under the following headings:

(1) Theology
Or religion,
Ethics, or moral virtues. } Taught by Nature; by Revelation.

(2) Geography.

(3) My Profession.
1. Botany.
2. Pharmacy.
3. Nosology.
4. Anatomy.
5. Surgery.
6. Chemistry.

(4) Logic.

(5) Language.
1. English.
2. French.
3. Latin.
4. Greek.
5. Italian.
6. Spanish.
7. Hebrew.

(6) Physics.
1. The doctrines and properties of natural bodies.
2. Of the operations of nature.
3. Of the doctrines of fluids.
4. Of the properties of organized matter.
5. Of the organization of matter.
6. Astronomy.
7. Mechanics.
8. Rhetoric and Oratory.
9. History and Chronology.
10. Mathematics.

The subjects discussed include: Hints towards the Investigation of Truth and Political Opinions, On the Immortality and Immateriality of the Soul; Body; Organized Matter; On Governments; On the credulity of Mortals; An Essay to prove that the Thinking Powers depend on the organization of the Body; A Defence of Materialism; An Essay on the ultimate End of Being; On Happiness; On Moral Obligation. Later, he turned the book round, and his opinions also, for the further essays are on Theology; The Christian Religion not repugnant to True Philosophy; An Essay on the Influence of Climate on National Manners and Happiness; On Friendship, an Essay; some verses and the beginning of a romance named "An Idyl," a dialogue about "Trevelis, a warrior." There is evidence that he had read the works of Locke, Hartley, Hume, Berkeley, Helvetius,

Condorcet and Reid; and knew something of Kant. The discussion of materialism contains passages of the following sort: "If we trace the progress of the thinking powers from their original source, we shall find that they owe their being to perception. A child, when it first comes into the world, is without ideas, and, consequently, he does not think. All the actions he performs arise from instinct. When hunger calls him, he satisfies his cravings with the milk of his mother; nor does he at all differ from the most stupid animal, only in being more helpless. He possesses but a small degree of perception; his attention is awakened with difficulty; the memory is weak and faint; and the ideas, without being often repeated, are not retained. As the child advances in years, the nerves become firmer and the brain stronger; perception is quicker, and the memory is more tenacious and retentive. Judgment, the result of perception and memory, is displayed by degrees, reason as slowly advances; and, lastly, disposition, the boundary of human intelligence, appears. Gradual is the progress of mind from sense to science. When the mental faculties have reached their highest perfection in manhood, they gradually decline; and nought is left of all the wreck of human knowledge but pure sensation, a principle gradually decaying with the falling frame. From hence there follows a self-evident corollary, that the thinking powers are not always the same: whatsoever is not always the same is naturally changeable, is mortal and material. Besides, we have traced the power of thinking from 0 to 0, increasing with the corporeal powers, and decreasing and ending with them."

Within a few months or a year he noted the following points as a basis for religious belief.

(1) The power of thinking does not naturally belong to matter.

(2) Motion, if ever so artfully distributed, it is plain, can produce nothing but motion.

(3) Matter acts only in proportion as it moves; thinking is acting without motion; *ergo*, that which thinks is not matter.

(4) The universality of the hypothesis.

(5) Internal consciousness of the existence of a monadic indivisible soul.

A little later, he based his religious beliefs on the consideration of final causes, after the manner of Isaac Newton. God was necessary in order to create and set in motion the mechanism of nature. Without the direction of a powerful, active and intelligent Being, the world must have been the product of chance, and the production of rational beings and geometrical order out of a universe of arbitrarily dancing atoms was inconceivable.

Then he wrote "A Letter on the pretended Inspiration of the Quakers and other Sectaries" in which he discussed what he conceived to be the illusions of such sects. He concluded that "a moderate degree of rational scepticism" was necessary in the examination of religious opinions. The reader will not be surprised to learn that after Davy had demonstrated his wonderful lecturing gifts, powerful members of the Church of England invited him to join its ministry, no doubt with the prospect of a bishopric. He had the correct attitude.

In 1795 he conceived a poem named: "The Sons of Genius." It was finished a year or two later and included in the *Annual Anthology*, edited by Southey and published in 1799, after he had gone to Clifton, and become acquainted with Southey, Coleridge, Wordsworth and their circle. Davy naturally found himself in the literary movement named the Romantic Revival. He was the leading British scientific figure in the social development of which the Romantic Revival was the literary expression. His grandmother's tales of superstition and horror were to him, perhaps, what Monk Lewis's stories were to Shelley. His lasting nature-worship made him perfectly comfortable with Wordsworth. The last five stanzas of "The Sons of Genius" exhibit the style of his verse:

> Like the tumultuous billows of the sea
> Succeed the generations of mankind;
> Some in oblivious silence pass away,
> And leave no vestige of their

Others, like those proud waves which beat the shore,
A loud and momentary murmur raise;
But soon their transient glories are no more,
No future ages echo with their praise.

Like yon proud rock, amidst the sea of time,
Superior, scorning all the billows' rage,
The living Sons of genius stand sublime,
The immortal children of another age.

For those exist whose pure ethereal minds,
Imbibing portions of celestial day,
Scorn all terrestrial cares, all mean designs,
As bright-eyed eagles scorn the lunar ray.

Theirs is the glory of a lasting name,
The meed of genius, and her living fire;
Theirs is the laurel of eternal fame,
And theirs the sweetness of the muse's lyre.

Davy's fluency is illustrated by his composition of a prologue to the comedy *The Honey Moon*, produced at Drury Lane in 1805. An acquaintance called on him while he was busy in the laboratory of the Royal Institution. He mentioned that the author was dead and no prologue had been obtainable from the professional poets, and the play was to be produced on the following evening. Davy instantly left the laboratory and returned two hours later with a prologue of forty-nine lines. The closing lines are:

Whether his talents have his wish belied;
Your judgment and your candour must decide.
He, though your loftiest plaudits you should raise—
He cannot thank you for the meed of praise.
Rapture he cannot feel, nor fear, nor shame;
Connected with his love of earthly fame,
He is no more—yet may his memory live
In all the bloom that early worth can give!
Should you applaud, 'twould check the flowing tear
Of those to whom his name and hopes are dear.
But should you an unfinish'd structure find,
As in its first and rudest forms design'd
As yet not perfect from the glowing mind,
Then with a gentle voice your Censure spread,
And spare the living—spare the sacred DEAD!

Coleridge said that if Davy had not been the first chemist, he would have been the first poet of the age. This criticism does more credit to Coleridge's friendship than his acumen as a critic. The quotations from Davy's verse are typical, and show plainly that he had no poetic talent. As his effective voice lacked musical quality, his fluent writings lacked literary quality. Davy's conventional literary compositions are commonplace, his writing shows good taste only when he is not being literary, and greatness only when he is using it to describe new discoveries, and the significance of science. Coleridge said, also, that he attended Davy's lectures in order to increase his stock of metaphors. This was a more judicious statement, as Davy was continually describing new knowledge and new significances of old knowledge. A poet might easily have found suggestion in Davy's scientific activities.

The failure of the Romantics and conventional poets to use scientific knowledge as a material of poetry would be an interesting subject of research. Wordsworth wrote a long essay on the problem and explained why there could be no contemporary combination of science and poetry. He considered poetry could be written only about ideas and things which had become familiar through long acquaintance, and part of the unconscious cultural equipment of the reader. He thought that poetry might be written about science when scientific conceptions became psychologically as familiar as natural objects to men. The world would have been saved many volumes of useless verse if writers of traditional poetry had digested Wordsworth's arguments. Electrons and positrons cannot be described in a poetic style appropriate to the description of Greek heroes, or sunsets. Literature must approach them with a new intellectual perspective and technique. It is possible that the first tentative pieces of genuine poetry in English on scientific subjects have been written by modern poets such as Spender and Bottrall.

Davy's verse resembles the uninspired part of Coleridge's verse. None of his literary prosewriting is as good as his wife's last letter to him. In reply to a

letter from her husband, who was dying in Italy, she wrote:

"I have received, my beloved Sir Humphry, the letter signed by your hand, with its precious wish of tenderness, bearing date the 1st of March. I start to-morrow, having been detained here by Drs. Babington and Clarke, till to-day. I shall travel with all the expedition I can, to arrive not quite useless. I trust still to embrace you, for so clear and beautiful expressions and sentiments cannot be the inhabitants of decay, however of feeble limbs and frame. I shall to the extremest point hold your wishes sacred, and obey in ready willingness the spirit even more than the letter of your order. God still preserve you, and know that the lofty and noble tone of your letter deepens all love and faith I have ever borne to you, and believe the words of kind effort will be a shield to me through life. I cannot add more than that your fame is a deposit, and your memory a glory, your life still a hope.

Your ever faithful and affectionate

JANE DAVY."

The able, chilly, intellectual tone of this letter would be less comforting to most dying men than a simple expression of human feeling, but it was one of the qualities in his wife which attracted him when young. It is difficult to believe she wrote the letter without thought of future publication, but in it she showed more skill than her husband in any of his literary writings.

Davy wrote two letters, which will be quoted presently, that reveal his personal greatness completely; his reply to Faraday's inquiry for a position, and to the invitation to study mine dangers. But they are without pure literary interest.

Davy appears to have been an apothecary's apprentice before he became specially interested in chemistry. He writes that he "began the pursuit of chemistry by speculations and theories" and comments that "more mature reflection convinced me of my errors, of the limitations of our powers, and the dangers of false generalizations." He

probably began the experimental study of chemistry in 1798, when he was nineteen years old. After four months of research he formed original views on the nature of light and heat, and invented the classical experiment of melting ice by friction, which is the most striking demonstration that heat is not a fluid, but probably a mode of motion. He described his results in a paper entitled: *An Essay on Heat, Light, and the Combinations of Light*, about twenty thousand words long. It was published early in 1799 in *Contributions to Physical and Medical Knowledge, principally from the West of England, collected by Thomas Beddoes, M.D.*

Davy's sudden development of interest in chemistry may have been due to the remarkable event of his acquaintance with Gregory Watt, the son of James Watt. Gregory Watt was consumptive and went to Penzance in the winter of 1797 to enjoy the mild climate. He was clever, and had been well-educated at Glasgow University, especially in chemistry and mechanics. He happened to find lodgings in the house of the widow Davy. At first he disliked the uncouth, self-confident son of his landlady, but when he discovered the youth had a powerful intelligence he soon became attached to him. The friendship became very close, and lasted until Gregory's death in 1805. Thus Davy became acquainted with one of the chief sources of contemporary progress. He could have had no better introduction to the best spirit of his time.

Davy was not only acquainted with the Watts; he was intimately acquainted with their work. He lived in the midst of one of the chief breeding grounds of mechanical invention. It is often forgotten that Cornwall was one of the leading scenes of the birth of mechanical civilization. In the late eighteenth century it had a relative economic importance comparable with that of Lancashire or the Midlands to-day. The Carn Brea district even to-day looks like a Black Country and offers a picture of how the county of Durham will appear in fifty years' time. The profits of its important tin-mining industry provided the economic stimulus towards the improvement of mining efficiency, by the introduction of improved mechanical methods, especially

in pumping machinery. The remote geographical position of Cornwall produces the illusion that the district was also remote from the centres of contemporary industrial development. In Davy's time Cornwall was the scene of one of the most brilliant periods in the history of human invention. Newcomen had invented the steam pumping engine early in the eighteenth century. James Watt improved this engine by condensing the steam in a vessel outside the working cylinder. One of his earliest engines was installed for the remarkable Wherry Mine at Penzance. The pumping engine was on the land, but the shaft of the mine was in the sea a mile from the shore. It was approached by a wooden bridge, and its workings, which provided Davy and Gregory Watt with a rich variety of minerals, were entirely under the sea. It was destroyed by a ship which broke from its anchorage and rammed the oversea machinery.

A few years after Davy left Cornwall, William Murdock invented coal-gas lighting in Redruth. Richard Trevithick, the inventor of the high-pressure steam-engine, the builder of the first steam railway locomotive, the inventor of the Cornish boiler, and many other machines, was rearing his turbulent genius in Cornwall. Trevithick's friend and theoretical adviser was Davies Giddy, the wealthy amateur and Cornish landowner, who had studied mathematics at Cambridge. Giddy changed his name to Gilbert, and succeeded Davy in the Presidential Chair of the Royal Society. It is said that Davy's talent for "pulling faces" secured his introduction to Gilbert, who happened to notice him as he was carelessly swinging on the gate of Mr. Borlase's house. Gilbert's companion remarked that the extraordinary looking boy was said to be fond of making chemical experiments. Gilbert invited him to his house and offered him the use of his library. He took him to see the laboratory at the Hayle Copper-House. Davy examined the chemical apparatus, examples of which he had seen before only in engravings, with tumultuous delight.

During his Penzance days, Davy also made the acquaintance of Josiah Wedgwood, and Thomas Wedgwood

whom he helped later in his pioneer experiments in photography.

Davy communicated his new theories of Heat and Light, based on a few months' study, to Dr. Beddoes at Clifton, Bristol. Beddoes was a medical doctor and had been professor of chemistry at Oxford, and had translated Sheele's *Chemical Essays*. He was a learned and original man, and was converted to Davy's views. Beddoes believed that the respiration of gases would be of immense value in the treatment of disease. The existence of gases different from air, established by Black and his successors, was still an astonishing discovery. The extraordinary properties of oxygen had stimulated much medical speculation. Beddoes wished to establish an institution for the experimental investigation of gases, especially in connection with physiology. He succeeded in collecting subscriptions for this purpose, and then considered the appointment of a superintendent of experiments. He thought of Davy, who had written to him in 1798, describing his new theory of heat and light, and offered him the appointment. Davy was tremendously excited by the prospect, but he was not too excited to insist several times on what he named a "genteel competence." Davies Gilbert negotiated the agreement between Beddoes and Davy.

On October 2nd, 1798, Davy left Penzance, before he was twenty years old. He was in the highest spirits, and his enthusiasm was increased by the arrival of the news, during his journey, of Nelson's victory of the Nile. Davy started his professional scientific career at the beginning of the British trading and industrial class's triumph. The Battle of the Nile was a sign of the rise of the British industrial development of the nineteenth century. At the very moment when the news of the victory was travelling through the world, the inspired leader of the scientific side of this development was travelling to the scene of his first internationally famous discovery.

When Davy had been established superintendent of the Medical Pneumatic Institution at Clifton, he was expected to engage in relevant experiments. He writes that in

March, 1798, before he had left Penzance, a short time after he had begun to study chemistry, his attention was directed to Priestley's dephlogisticated nitrous gas (nitrous oxide) by Dr. Mitchill's theory of Contagion. According to Mitchill sepsis was produced by nitrous oxide, which he named oxide of septon. Davy soon satisfied himself, by a few experiments, that this theory was fallacious. "Wounds were exposed to its action; the bodies of animals were immersed in it without injury; and I breathed it, mingled in small quantities with common air, without any remarkable effects. An inability to procure it in sufficient quantities prevented me, at this time, from pursuing the experiments to any greater extent. I communicated an account of them to Dr. Beddoes." Further investigation of this subject seemed an obvious task for the new Institution.

Beddoes himself tried strange experiments. Maria Edgeworth was his sister-in-law and has recorded that "one of his hobbies was to convey cows into invalids' bedrooms that they might inhale the breath of the animals, a prescription which naturally gave umbrage to the Clifton lodging-house keepers, who protested that they had not built and furnished their rooms for the hooves of cattle."

Meanwhile, the *Essays on Heat and Light* were published. Nine-tenths of them are ingenious and fallacious speculation, but one-tenth shows high genius.

In the section entitled: "The Phenomena of Repulsion are not Dependent on a Peculiar Elastic fluid for their Existence, or Caloric does not Exist," he argues that the expansion in objects when heated is not due to the absorption of caloric, which fills the spaces between the atoms and makes the object swell. If heat is a form of matter, then the temperatures of bodies cannot be increased unless they gain heat through contact with other bodies, or their own capacity for heat is reduced by some treatment. It is known that friction raises the temperature of bodies. If the caloric theory is true, then friction must be able to decrease the capacity of a body for heat. When the capacity is reduced, the temperature of the body will rise. Davy arranged a

cold machine for rubbing two pieces of ice together violently. He writes: "The fusion took place only at the plane of contact of the two pieces of ice, and no bodies were in friction but ice. From this experiment it is evident that ice by friction is converted into water, and according to the supposition (that heat is a form of matter) its capacity is diminished; but it is a well-known fact that the capacity of water for heat is much greater than that of ice; and ice must have an absolute quantity of heat added to it, before it can be converted into water. Friction consequently does not diminish the capacity of bodies for heat."

Hence heat cannot be a form of matter. After some more elucidatory experiments he concludes: "Heat, then, or that power which prevents the actual contact of the corpuscles of bodies, and which is the cause of our peculiar sensations of heat and cold, may be defined a peculiar motion, probably a vibration, of the corpuscles, tending to separate them. It may with propriety be called the repulsive motion." He defines light as particles of matter in the condition of *repulsive projection*. "The violence of this repulsive condition is shown by the velocity of the recession of the particles." In a cancelled note he writes that the electric fluid is condensed light. The aurora borealis is due to condensed light which has been converted into light again by the revolution of the earth. "No more sublime idea can be formed of the motions of matter, than to conceive that the different species are continually changing into each other. The gravitative, the mechanical, and the repulsive motions, appear to be continually mutually producing each other, and from these changes all the phenomena of the mutation of matter probably arise."

He contends that light "enters into the composition of a number of substances. In some of these, the incombustible phosphorescent bodies, it most probably exists in a state of loose combination." He says oxygen gas contains light, and should be named phosoxygen. The light that appears during the combination of phosoxygen and hydrogen is released from the phosoxygen. "Water is decomposed by two attractions; that of light for oxygen, and of a certain

hydrogen attractor for hydrogen." He then describes experiments on sea-weed (from the shore at Penzance) which, he considers, prove that it can decompose water by attracting its hydrogen.

In his investigation of the role of phosoxygen in the respiration of fishes he writes in a footnote:

"I have discovered by similar experiments that the zoophyta are governed by similar laws: that they, like fish, absorb the phosoxygen held in solution by water, as well as portions of nitrogen; and thus in their chemical attractions, as well as in their organic powers, seem to be the connecting links between vegetables and animals."

This was the first recorded investigation of the respiration of the zoophyta. He supposes that the brain receives the electric fluid, which is condensed light, through the nerves. "The muscles are most probably phosoxydated compounds, of which the numerous principles are in exact and delicate equilibrium; and it is likely that on this equilibrium their irritability depends." In a footnote he remarks: "The torpedo, and some other animals, give out electric fluid during animal action. In man, the quantity is probably, however, too small and too slowly liberated to be ascertainable. It would be worth while to try, by a very sensible electrometer, whether an insulated muscle when stimulated into action, would not give marks of the liberation of electric fluid."

He concludes his essay on heat and light with the remark that every change in sensations and ideas must be accompanied by some corresponding change in the organic matter of the body. Experimental investigation should elucidate these changes and provide the means for destroying pain and increasing pleasure. "Thus would chemistry, in its connection with the laws of life, become the most sublime and important of all sciences."

In the following essay, on the causes of the colours of organic beings, he ascribes the bright colours of various minerals to their high content of phosoxygen. "The whiteness of etiolated vegetables is occasioned by the deficiency of light; the different shades of green in the leaves of

vegetables depend on the light entering into their composition; and the fine colours of the different flowers appear to be produced by combined light." He supports these opinions by experiments on lettuces and roses. "I have found by experiment that red rose trees, when carefully included from light before their flowers begin to appear, and supplied plentifully with water and carbonic acid, produce flowers almost white."

The colours of fish and birds are due to absorption of light. "The hair on the parts of beasts not exposed to light is uniformly paler than that covering the parts exposed to its influence; and it affords a striking proof of this theory, that some of the northern animals are dark-coloured in summer, and white or pale in winter." The inhabitants of Northern Europe are white, of temperate America brown, and of the tropics black; in the order of the strength of the light. "Women, who are less exposed to light, are fairer than men."

Almost immediately after the publication of these essays, Davy deprecated them. He described them as his "infant speculations," and did not care to be reminded of them afterwards. The numerous fallacies in them made him react against speculation. Henceforth his researches were severely disciplined by fact; perhaps too severely, as he became unsympathetic to general theories, such as the atomic theory of Dalton. In the introduction to his *Researches on Nitrous Oxide*, published in 1800, he writes: "Early experience has taught me the folly of hasty generalization. We are ignorant of the laws of corpuscular motion; and an immense mass of minute observations concerning the more complicated changes must be collected, probably before we shall be able to ascertain even whether we are capable of discovering them. Chemistry, in its present state, is simply a partial history of phenomena, consisting of many series more or less extensive of accurately connected facts."

After the brilliant maturity of this passage is admitted, one cannot help wondering whether Davy's haste to reject his *Essays* was not influenced by his desire not to impede

his ambition by maintaining unpopular views. Though Davy quickly lost his pride in his *Essays*, their themes made other converts besides Beddoes. The distinguished and remarkable Joseph Priestley, then about sixty-seven years old, who was living in Northumberland, U.S.A., a refugee from persecution, wrote in the appendix to his last chemical work, *The Doctrine of Phogiston Established:* "When some progress was made in printing this work, I met with Dr. Beddoes's Contributions to Physical Knowledge, and in it, Mr. H. Davy's Essays, which have impressed me with a high opinion of his philosophical acumen. His ideas were to me new, and very striking, but they are of too great consequence to be decided upon hastily."

What a notice for the product of a Penzance youth's first few months' *study* of chemistry! The essays were not the product of the first few months' research after several years' study of elementary chemistry, they arose out of the first few months' acquaintance with the subject.

After Davy went to Clifton, Priestley's son studied with him. In 1801, three years before he died, Priestley wrote to Davy from America:

"Sir, I have read with admiration your excellent publications, and have received much instruction from them. It gives me peculiar satisfaction that, as I am far advanced in life, and cannot expect to do much more, I shall have so able a fellow-labourer of my own country in the great fields of experimental philosophy. As old an experimenter as I am, I was near forty before I made any experiments on the subject of Air, and then without, in a manner, any previous knowledge of chemistry. This I picked up as I could, and as I found occasion for it, from books. I was also without apparatus, and laboured under many other disadvantages. But my unexpected success induced the friends of science to assist me, and then I wanted for nothing. I rejoice that you are so young a man, and perceiving the ardour with which you begin your career, I have no doubt of your success." Priestley then refers to Davy's friendship for his son, and asks him to inform him of the progress of science in Europe. "I am here perfectly

insulated, and this country furnishes but few fellow-labourers, and these are so scattered, that we can have but little communication with each other, and they are equally in want of information with myself." He corrects an inadvertent error in one of Davy's papers, and signs himself "With the greatest esteem,

I am, Sir, yours sincerely,

J. PRIESTLEY."

Davy was now just twenty years old, established as a professional scientist, and superintendent of a research institution. His advantages did not end there. Dr. Beddoes' house was the centre of an exceptional intellectual society. His wife was the sister of Maria Edgeworth. She was a charming woman, and kind to Davy, who became very fond of her. She probably improved his uncouth manner. Southey, Coleridge, Wordsworth, Tobin and the publisher Cottle were among the Beddoes' friends. The poets were in their twenties, and at the height of their genius. *Lyrical Ballads* had just been published. Cottle writes of his introduction to Davy: "I was much struck with the intellectual character of his face. His eye was piercing, and when not engaged in converse, was remarkably introverted, amounting to absence, as though his mind had been pursuing some severe train of thought scarcely to be interrupted by external objects; and, from the first interview also, his ingenuousness impressed me as much as his mental superiority."

Davy was magnificently happy in his fine new rooms, laboratory equipment and friends. He started almost immediately on the study of gases for the Pneumatic Institution, by extending the experiments with nitrous oxide he had made at Penzance, shortly after he had begun the study of chemistry in March, 1798. He had discontinued these experiments because he had not been able to obtain sufficient quantities of the gas. At Clifton he started an attempt to prepare larger quantities, in a pure state, and succeeded after a few weeks, in April, 1799. He resolved to attempt to inspire it, as he saw no other way in which

respirability could be determined. This decision, in the face of Mitchill's theory of nitrous oxide as the cause of contagion, shows Davy's confidence in his own Penzance experiments disproving Mitchill's theory, and his personal courage. After a number of attempts he succeeded in respiring nitrous oxide on April 17th, in the presence of Dr. Beddoes. "Having previously closed my nostrils and exhausted my lungs, I breathed four quarts of nitrous oxide from and into a silk bag. The first feelings were similar to those produced in the last experiment; but in less than half a minute, the respiration being continued, they diminished gradually, and were succeeded by a sensation analogous to gentle pressure on all the muscles, attended by a highly pleasurable thrilling, particularly in the chest and the extremities. The objects around me became dazzling and my hearing more acute. Towards the last inspirations, the thrilling increased, the sense of muscular power became greater, and at last an irresistible propensity to action was indulged in."

The remarkable discovery was explored with intense thoroughness. He breathed the gas three or four times a day for weeks together, he attempted to observe its general effects on his health, and cautiously allowed for the psychological effects of the interest and labour of the investigations. Davy's sudden development of speculative reserve after leaving Penzance may have been helped by the reactions against the wild speculations of his elder, Beddoes. The founder of the Pneumatic Institute soon convinced himself that nitrous oxide was beneficial for paralytics. Davy's naturally sanguine temperament was sobered by the easy acceptance of such convictions.

About July he notes: "In cutting one of the unlucky teeth called *dentes sapientiæ*, I experienced an extensive inflammation of the gum, accompanied with great pain, which equally destroyed the power of repose, and of consistent action. On the day when the inflammation was most troublesome, I breathed three large doses of nitrous oxide. The pain always diminished after the first four or five inspirations; the thrilling came on as usual, and

uneasiness was for a few minutes swallowed up in pleasure. As the former state of mind, however, returned, the state of organ returned with it."

This was the first observation of the value of nitrous oxide as an anæsthetic and in connection with the teeth. It arose out of the arrival of the young philosopher's wisdom tooth; rather late after the arrival of his wisdom! Near the end of the monograph he writes: "As nitrous oxide in its extensive operation appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place."

Unfortunately, the preliminary stimulating effect of the gas, which made the respirer laugh and stamp, attracted more attention. The effects of "laughing gas" became a turn in shows, and Davy's very clear suggestion of its anæsthetic value was not followed for no less than forty-four years, when an American dentist named Horace Wells demonstrated its value in the extraction of teeth in 1844. The Americans are the founders of modern anæsthesia, their chemist, C. T. Jackson, demonstrated ether was a narcotic, in 1841. The American physician, W. C. Long, experimented with it, and the American dentist, W. Morton, demonstrated its value in extractions in 1846. His success inspired the Boston surgeon, J. C. Warren, to use it for long operations. These achievements were followed by J. Y. Simpson's introduction of chloroform at Edinburgh in 1847.

When Beddoes was dying he wrote to Davy asking for his consolation. He regretted that his scientific researches had failed, and that he had indulged in many scientific aberrations. History proved that he was unfortunate, for his belief in the medical importance of respiratory gases was justified, and the young man to whom he had given such wonderful opportunities had within a year provided sufficient evidence of its truth.

The failure of the contemporary physiologists to adopt Davy's discovery offers an interesting subject of research. The ancient medical profession was perhaps disinclined to

attend to the discoveries of persons outside its own ranks. Medicine had immense prestige, and regarded chemistry as socially inferior. The conventional view of the status of chemistry relative to other intellectual activities is seen in Francis Horner's comments on Davy in 1802. He wrote: "I have since met Davy in company, and was much pleased with him; a great softness and propriety of manner, which might be cultivated into elegance, his physiognomy struck me as being superior to what the science of chemistry, on its present plan, can afford exercise for."

Davy did not abandon his intention to graduate as a medical doctor until he was twenty-nine years old, and had already for two years been one of the two secretaries of the Royal Society, during which time he was registered as an undergraduate and kept terms at Cambridge ; a situation which appears incredible to-day. He accepted Beddoes' invitation to Clifton on the understanding that he would be able to acquire clinical experience during his work for the Pneumatical Institution.

The debt of chemistry to physiology is to-day frequently forgotten. The money which financed Davy's first professional research was subscribed for medical objects; the prestige of medicine provided a shelter for chemical research even when its pride prevented the acceptance of the products of this research.

After his success with nitrous oxide Davy attempted to respire various gases, including hydrocarbonate, or water-gas, made by passing steam over charcoal. Of the latter he writes: "After the second inspiration, I lost all power of perceiving external things, and had no distinct sensation except a terrible oppression on the chest. During the third expiration, this feeling disappeared, I seemed sinking into annihilation, and had just power enough to drop the mouth-piece from my unclosed lips. . . . On recollecting myself, I faintly articulated 'I do not think I shall die.' He felt his pulse and found it thread-like and excessively quick. He staggered into the garden and collapsed on the grass. He asked for nitrous oxide, and reports that he *believed* himself relieved. After a very bad afternoon, when he noted his

pulse was 120, and a splitting headache, he became better in the evening, awaking next morning weak and hungry. Of this experiment he calmly concludes: "There is every reason to believe, that if I had taken four or five inspirations instead of three, they would have destroyed life immediately without producing any painful sensation."

Coleridge, Southey, Wedgwood, Tobin, the Edgeworths and many others recorded their sensations while breathing nitrous oxide. Davy's discovery immediately became famous, as there is a wide interest in things that affect the human body. The Pneumatic Institution received many new patients, and prospered.

While investigating the physiological properties of the gas, and demonstrating them with wonderful skill, Davy researched with extreme intensity on the chemistry of the oxides of nitrogen. In ten months he had collected many new facts. He showed that the decomposition of ammonium nitrate can occur in several ways and is accompanied by absorption of heat. He gave a very tolerable estimate of the percentages of nitrogen and oxygen in nitrous oxide, and methods of obtaining the gas pure. He discusses the composition of the air, and supposes it a compound of oxygen and nitrogen.

The purely chemical part of his work on nitrous oxide showed a brilliant talent, but not a genius. Davy's highest gifts are not seen in systematic analysis, or in a logical comprehension of a variety of facts within a general theory. They are seen in isolated experiments whose results enabled him to throw light into new regions of phenomena. He was a breaker of obscurity. The surveying and classifying of the hosts of new phenomena revealed by his raiding intellect was better done by others.

Davy collected the new chemical and physiological facts for his treatise on *Nitrous Oxide* in ten months, and wrote his description of them, a book containing over eighty thousand words, in three months. Before his astonishing book had left the press he had already found the next subject for the exercise of his genius. During the same period he read widely, wrote various essays, and much verse.

He enjoyed walking and shooting in the countryside. He walked on the more "sublime" parts of the hills at Clifton, composing verse while breathing nitrous oxide from a bag, hoping that the gas might improve his poetry. The result was:

Not in the ideal dreams of wild desire
Have I beheld a rapture-wakening form:
My bosom burns with no unhallow'd fire,
Yet is my cheek with rosy blushes warm;
Yet are my eyes with sparkling lustre fill'd;
Yet is my mouth replete with murmuring sound;
Yet are my limbs with inward transports fill'd;
And clad with new-born mightiness around.

The physiological interest of this composition is clearly greater than the poetical. Coleridge has described how he composed *Kubla Khan* after taking opium to relieve pain. Perhaps Davy's experimental investigation of the influence of nitrous oxide on the poetical inspiration suggested opium might have inspirational effects. Coleridge's composition under the influence of opium was wonderfully inspired, in contrast with Davy's under the influence of nitrous oxide.

The great events in Davy's life during the years 1799 and 1800 were not restricted to his enormous labours on nitrous oxide, to his acquaintance with the Romantic poets and to cultivated social life. He heard of Volta's discovery of the electric pile, or battery, which had been communicated by the discoverer in a letter from Como to Sir Joseph Banks, the President of the Royal Society. Nicholson and Carlisle had constructed a pile on April 30th, and discovered that water could be decomposed by electricity on May 2nd. Davy at once appreciated the importance of Volta's discovery and started experiments on electrolysis. His first results convinced him of the fundamental connections between electricity and chemical affinity.

The appreciation of the importance of the Voltaic pile was so swift that the brilliant applications of Nicholson and Carlisle, and the observations by Cruickshank and Henry were published before Volta's original description of the

pile had been published in the *Philosophical Transactions* of the Royal Society. Davy's first paper on electro-chemistry was published in *Nicholson's Journal* in September, 1800. Within six months he published six papers on the subject. In a letter dated October 20th he writes to Davies Gilbert that he has discovered galvanism is a purely chemical process depending wholly on the oxidation of metallic surfaces. Zinc is incapable of decomposing pure water; if the zinc plates are kept moist with pure water, the pile will not work. The water must contain in solution air, oxygen, nitrous acid, muriatic acid, etc. Following the observation of Haldane of the "non-excitement of galvanism in the vacuum of an air pump" he found the pile acts for a few minutes only when introduced into hydrogen, nitrogen, or hydrocarbonate; only as long as the water between the plates holds some oxygen in solution. The pile recovers when immersed in air. It acts very vividly in oxygen.

He found oxygen and hydrogen could be separately produced from quantities of water not immediately in contact. If one terminal of the pile was connected to one glass of water, the circuit was successfully completed by his body when he put the fingers of his right hand in one glass and those of his left in the other. Three people holding hands could successfully complete the current. The circuit could be completed with muscular fibres, vegetable fibres, and moistened threads not exceeding three feet in length. "Muscular fibre appeared to be a better conductor than vegetable fibre, and vegetable fibre a better conductor than moistened thread." He suggested that the nascent hydrogen released in various decompositions would be a "powerful and accurate instrument of analysis." When he had concluded that the electric current from the pile was dependent on the degree of chemical activity, and not on the decomposition of water, he immediately constructed a new form of pile using muriatic and other acids between the plates. Concentrated nitrous acid gave a shock so powerful that his fingers were benumbed for some seconds and he did not dare to take another.

"Sulphuric acid, when highly concentrated, is possessed of but little power of action on zinc, though when diluted it dissolves it with the greatest rapidity. Assuming then the truth of the principles advanced in my last paper, namely, that the powers of the pile of Volta are primarily excited by the oxydation of the zinc, it follows, that diluted sulphuric acid, when made the medium of connection between the pairs of plates, ought to produce much greater effects than concentrated sulphuric acid. This I have found is actually the case."

He "found that an accumulation of galvanic influence, exactly similar to the accumulation in the common pile, may be produced by the arrangement of single metallic plates, or arcs, with different strata of fluids."

The beginning of the inquiries, which finally brought Davy to the Royal Institution in London, was the second event in 1800, as important for him as the invention of the Voltaic pile. Indeed, it was more important. He made the success of the Royal Institution, and so secured recognition for the new sociological objects for which it was founded. He became the chief voice of the movement of social development based on the application of science to production. His sociological significance exceeded the scientific significance of his great discoveries.

The Royal Institution was founded in March, 1799, through the initiative of Count Rumford. The original name of this remarkable man was Benjamin Thompson. He was an American of British ancestry. In his youth he was poor, and was apprenticed to a business. He soon left business life, and became a teacher. In spite of his early departure from commercial into academic life, he rapidly became wealthy, as he married a rich widow when he had become a teacher and was no longer a business man. He enlisted in the American Army when the War of Independence started, but he had Royalist sympathies and deserted to the enemy, and fled in a British ship. He never saw his wife again, and did not see his baby daughter until twenty years later. His charming manner and ability secured him promotion in the British service and he became Under-

Secretary of State for the Colonies. He later enlisted in the service of the Kurfürst Karl Theodor of Bavaria, who made him a count of the Holy Roman Empire, a title Thompson always used afterwards. He reorganized Karl Theodor's War Office and became his Minister for War. As part of his military duties he also had to reorganize the Munition Works in Munich, and during this task he noticed the remarkable production of heat in guns when they were being bored, especially with blunt borers. He investigated the effect carefully and collected good evidence against the theory that heat is a material fluid. In Munich he encouraged the improvement of fire-grates and cooking utensils, and the application of science to the common things of life. He laid out the "English Garden," in which a memorial was raised to him in 1795. Rumford was often in London on diplomatic missions and continued his interests there. In 1796 he made a "proposal for forming in London by private subscription an establishment for feeding the poor, and giving them useful employment, and also for furnishing food at a cheap rate to others who may stand in need of such assistance, connected with an institution for introducing and bringing forward into general use new inventions and improvements, particularly such as relate to the management of heat and the saving of fuel, and to various other mechanical contrivances by which domestic comfort and economy may be promoted."

Rumford told his friends that he was "deeply impressed with the necessity of rendering it *fashionable* to care for the poor and indigent."

The Society for Bettering the Condition of the Poor was founded to meet the first suggestion in his agitation. The second suggestion, for the founding of a research institution, was separated from the first, as it would be "too conspicuous and too interesting and important, to be made an *appendix* to any other existing establishment, and consequently it must stand alone, and on its own proper basis." In 1799 the Institution was founded and private subscriptions were collected for "a public institution for diffusing the knowledge and facilitating the general and speedy introduction

of new and useful mechanical inventions and improvements; and also for teaching, by regular courses of philosophical lectures and experiments, the applications of the new discoveries in science to the improvement of arts and manufactures, and in facilitating the means of procuring the comforts and conveniences of life." Sir Joseph Banks, the President of the Royal Society, was the chairman of the managers, and Rumford was the secretary. A house was bought in Albemarle Street and its rooms converted into laboratories, lecture rooms, offices, etc.; and a flat for the accommodation of Rumford. "A good cook was engaged for the improvement of culinary advancement—one object, and not the least important—for the Royal Institution." Like all other institutions founded by social idealists, its character was rapidly adapted, not to the achievement of the precise objects of the founders, but to those objects in its constitution which were of interest to classes with increasing social power. As the class of student in the public grammar schools of the fifteenth century gradually changed from orphans to the sons of princes, and as the co-operative movement of the Rochdale pioneers changed from a communal society into a business paying dividends, so the Royal Institution changed from a laboratory for the solution of the problems of the poor into an institution for the solution of the scientific problems which governing opinion of the day thought important. The solutions of the problems of science ultimately benefit the poor, but only after they have benefited the industrialists who exploit science.

Thomas Garnett, the professor of chemistry in the Anderson's Institution in Glasgow, was invited in 1799 to join the staff of the Institution as Lecturer and Scientific Secretary. He was not very successful, so Rumford sought for a capable assistant or prospective successor. The brilliant young man at Clifton was an obvious candidate. Rumford began to inquire about him, and several scientists who knew him personally gave strong recommendations. His views on heat, at which he had arrived independently, were substantially the same as Rumford's.

Naturally, Davy was elated by the approach. In January,

1801, he wrote to his mother that he had received "proposals of a very flattering nature" to settle as assistant lecturer in chemistry, with the prospect of shortly becoming the sole professor in the Royal Philosophical Institution, "an appointment as honourable as any scientific appointment in the kingdom, with an income of at least 500 l. a year." . . . "You will all, I dare say, be glad to see me amongst the *Royalists*, but I will accept no appointment except upon the sacred terms of *independence*."

In February he went to London to negotiate with Rumford. He reported to his mother that the terms of the engagement had been settled satisfactorily, and that he had been kindly met by Sir Joseph Banks, Count Rumford and Mr. Henry Cavendish. He was appointed Assistant Lecturer in Chemistry, Director of the Laboratory, and Assistant Editor of Journals, with the allowance of a room, coal and candles for lodging, and a salary of one hundred guineas per annum; rather less than the five hundred pounds mentioned in his first letter, but a very fine sum for a man of his years and interests.

Paris writes that Rumford was not immediately impressed by Davy's personality and manners. Rumford had him lecture in the smaller theatre as a test. After hearing him speak he exclaimed: "Let him command any arrangements which the Institution can afford." Paris writes that "Davy's uncouth appearance and address subjected him to many other mortifications on his first arrival in London. There was a smirk on his countenance, and a pertness in his manner, which, although arising from the perfect simplicity of his mind, were considered as indicating an unbecoming confidence."

Davy had written to his mother that he was getting among the Royalists. Almost immediately after his appointment at the Royal Institution he was elected a member of the Tepidarian Society, whose membership consisted of twenty-five of the most violent republicans of the day. The name of the society arose from the tastes of the members, who at their meetings drank tea only. It is said that the swift growth of Davy's popularity was largely due to the Tepidarians.

They rallied their friends to his audiences before he had become well known.

His first lectures were devoted to various subjects. They were successful, but their success was transcended by the excitement in the following year. Garnett had lost his wife and become melancholic. He was asked to resign and Davy was promoted to be the Lecturer on Chemistry. On January 21st, 1802, he delivered the astonishing introductory lecture which has been discussed at the beginning of this chapter. It was attended by distinguished persons of all sorts. The Tepidarians held their anniversary dinner on the same day, in anticipation of the triumph of their fellow-member, and Davy attended a masquerade at Ranelagh in the evening. Compliments, invitations and presents were showered on him, ladies addressed anonymous poems to him, and he entered fashionable life under the auspices of the Duchess of Gordon. The most exclusive parties were considered incomplete without him. Davy was twenty-two years old. His manners and habits changed quickly. Paris records that "his vanity was excited, and his ambition raised by such extraordinary demonstrations of devotion, that the bloom of simplicity was dulled by the breath of adulation; and that, losing much of the native frankness which constituted the great charm of his character, he unfortunately assumed the garb and airs of a man of fashion." Some of Davy's friends were alarmed by these developments. The solicitations of Coleridge and Davy on behalf of each other's careers are touching and amusing. Coleridge wrote of Davy: "I see two Serpents at the cradle of his genius: Dissipation with a perpetual increase of acquaintances, and the constant presence of Inferiors and Devotees, with that too great facility of attaining admiration, which degrades Ambition into Vanity," while Davy wrote of Coleridge: "His eloquence is unimpaired; perhaps it is softer and stronger. His will is probably less than ever commensurate with his ability. Brilliant images of greatness float upon his mind. . . . I have looked to his efforts, as to the efforts of a creating being; but as yet he has not even laid the foundation for the new world of intellectual form."

In May, 1802, he was promoted to be Professor of Chemistry in the Royal Institution.

Davy carefully prepared his public lectures, and rehearsed the demonstrations, so that they were given with fluency and perfection. His preliminary attention to form allowed his natural earnestness and brilliance of manner to have their fullest effect. He worked with extreme rapidity. He composed his lectures in a few hours on the preceding day. In his habit he resembled the journalist, whose best work is usually not more than twenty-four hours old. It enabled him to give his discourse the freshest finish. As a public expositor his work needed the best qualities of journalism.

The method of his public discourses was in remarkable contrast with his conduct of research. In his laboratory he was slovenly. He corrected his notes by sticking his finger in the inkwell and blotting out the erroneous phrase. He usually ran several experiments at the same time. He passed from one to the other without obvious order and was perfectly reckless of his apparatus. He would break pieces of one to make some alteration in another which occupied his attention at the moment. Paris records his movements were so rapid that spectators thought he was preparing for an experiment when he was obtaining the results. "The rapid performance of intellectual operations was a talent which displayed itself at every period of his life." Poole wrote that the quickness and truth of his apprehension was his most striking characteristic. "It was a power of reasoning so rapid, when applied to any subject, that he could hardly be himself conscious of the process; and it must, I think, have been felt by him, as it appeared to me, pure intuition. I used to say to him: 'You understand me, before I half understand myself.'"

Davy was a remarkable example of the popular idea of a genius. This was one of the sources of his success.

Without him the Royal Institution would probably have collapsed. In 1800 the subscriptions were £11,047, but they fell in 1802 to £2,999. The first subscriptions for the Institution had naturally been the largest, and

persons were upset by Rumford's autocratic management. In 1803 Rumford left the Institution. He settled in Paris and married Lavoisier's widow. The marriage proved unhappy. Rumford's departure did not completely destroy the Institution's interest in technological research. Davy was instructed to investigate the technique of tanning, in order to improve the tanning industry. He was requested to investigate the chemistry of mineralogy and metallurgy. He studied all of these subjects with great energy and good will. Unlike many young intellectuals, he did not resent receiving instructions concerning the subjects of his researches. He was always glad to exhibit the versatility of his genius on any material, as his raiding intellectual temperament was adapted to making sudden attacks on new fields, or fields new to him. The products of his investigations of these technological subjects proved not to be very important.

The Board of Agriculture, under the inspiration of Arthur Young, invited him to give a course of lectures on the chemistry of agriculture. These attracted much attention and were profitable to Davy. He was asked to repeat them in Dublin. He was paid five hundred guineas for the course. In the next year the invitation was repeated, for a fee of seven hundred and fifty guineas. The tickets for these courses cost two guineas, and the applications greatly exceeded the seating accommodation in the lecture hall. The University of Dublin awarded him an honorary doctorate of laws; the only distinction conferred on him by any university.

Davy published his lectures as a treatise on agricultural chemistry. While he made no discovery of fundamental importance, he gave the science of agriculture dignity. He established its sociological prestige. He created the atmosphere in which Lawes and Gilbert could develop their great researches. Davy's services to culture are seen particularly clearly in his work for agricultural chemistry, for the spectator's attention is not distracted by brilliant discoveries. In considering his chief researches the spectator cannot always separate the sociological

from the scientific implications of his discoveries. In considering Davy's researches the student is often conscious of his weaknesses as an analytical chemist. This makes him underrate his importance because he measures him purely as a chemist, and not as a sociological phenomenon of which pure chemistry is only one aspect.

The variety of Davy's duties prevented him from concentrating on electro-chemistry during the first years at the Institution. He was elected to the Royal Society in 1803, though he had published one paper only in their *Transactions*. He received the Copley Medal, the Royal Society's highest honour, in 1805, for his researches in the chemistry of mineralogy. He was then twenty-six years old. While Davy's total performance had earned such distinction, his researches on mineralogy had not. The incident shows his power of inciting interest, for the students of mineralogy must have felt that his contributions were important, and have confused the interest he stimulated with the value of his discoveries in their subject.

During his early years in the Institution Davy used to enter the laboratory at about ten or eleven o'clock in the morning, and work until about four o'clock. He dined at five, and rarely returned to the laboratory in the evening. When he did not go to a party he played billiards, went to the theatre or read the latest novel. The intensity of his devotion to fashionable life never prevented him from punctual attendance at the laboratory in the morning, and the thread of his thought on his experimental researches was never broken.

At this period of his life he had no interest in money. He rejected the invitations of manufacturers and company promoters to advise directly, for large fees, on the improvement of manufacturing processes. "Any thing not immediately necessary to him he gave away, and never retained a book after he had read it."

In 1806 he described the state of electro-chemistry, and his own contributions to the subject, in the Royal Society's chief annual lecture on physical science: the Bakerian

Lecture. The first experimenters on the electrolytic decomposition of water had found that acids and alkalies appeared in the water during decomposition. Some supposed they were created by electricity out of water. Davy showed that they arose from impurities and dissolved air. Soda was dissolved from the glass of the apparatus, and nitrous acid arose from dissolved nitrogen combining with the nascent oxygen and hydrogen released during decomposition.

Hisinger and Berzelius had shown that if muriate of lime was placed in the positive part of a siphon electrified by wires from a Voltaic pile, and distilled water in the negative part, lime appeared in the distilled water. Davy deduced that "the saline elements evolved in decompositions by electricity were capable of being transferred from one electrified surface to another." Numerous experiments confirmed the deduction, and he noted that "the time required for these transmissions (the quantity and intensity of the electricity, and other circumstances remaining the same) seemed to be in some proportion as the length of the intermediate volume of water."

Hydrogen, the alkalies, the metals, and certain metallic oxides, are attracted by negatively electrified metallic surfaces; oxygen and acid substances are attracted by positively electrified metallic surfaces. These forces are "sufficiently energetic to destroy or suspend the usual operation of elective affinity. It is very natural to suppose, that the repellent and attractive energies are communicated from one *particle to another particle* of the same kind, so as to establish a conducting chain in the fluid; and that the locomotion takes place in consequence. . . ." "In the cases of the separation of the constituents of water, and of solutions of neutral salts forming the whole of the chain, there may possibly be a succession of decompositions and recompositions throughout the fluid." Davy confirmed Wollaston's discovery that water could be decomposed by electricity from a frictional machine, which showed the identity of frictional and Voltaic electricity.

Davy remarks that the experiments of Bennet had shown

that many bodies brought into contact and then separated "exhibited opposite states of electricity." He compared this phenomenon with the behaviour of metals and solutions in contact, and suggested the electrical separation connected with electrolysis was related to contact electricity. "Oxygen and hydrogen ought to possess, with regard to the metals respectively, the negative and positive energy. This I have not been able to prove by direct experiments of contact; but the idea is confirmed by the agency of their compounds."

Davy saw that there were general relations between contact electricity and electrolysis. He then remarked on the probable relationship of the phenomena of chemical combination, i.e. chemical contact, with contact electricity and the phenomena of electrolysis. "The different bodies naturally possessed of chemical affinities appear incapable of combining, or of remaining in combination, when placed in a state of electricity different from their natural order." For instance, zinc, which is easily oxidized, will not combine with oxygen when negatively electrified in the circuit, while silver, which is one of the least oxidizable, easily unites with oxygen when it is positively electrified. "Amongst the substances that combine chemically, all those, the electrical energies of which are well known, exhibit opposite states . . . and supposing perfect freedom of motion in their particles . . . they ought . . . to attract each other in consequence of their electrical powers. In the present state of our knowledge, it would be useless to attempt to speculate on the remote cause of the electrical energy, or the reason why different bodies, after being brought into contact, should be found differently electrified; its relation to chemical affinity is, however, sufficiently evident. May it not be identical with it, and an essential property of matter?" Davy proceeded to interpret the strength of chemical affinity in terms of the degree of the electrification of the constituent particles of compounds. Compounds containing three constituents were due to the balance of the electrical attractions and repulsions between the constituents. He then remarked: "The general idea will, however, afford an easy explanation of the influence of affinity by the masses

of the acting substances, as elucidated by M. Berthollet; for the combined effort of many particles possessing a feeble electrical energy, may be conceived equal or even superior to the effect of a few particles possessing a strong electrical energy."

An electrical theory of Mass Action!

In the next paragraph he writes: "Allowing combination to depend upon the balance of the natural electrical energies of bodies, it is easy to conceive that a *measure* may be found of the artificial energies, as to intensity and quantity produced in the common electrical machine, or the Voltaic apparatus, capable of destroying this equilibrium; and such a measure would enable us to make a scale of electrical powers corresponding to degrees of affinity."

Then the action of the Voltaic cell is explained: "When a communication is made between the two extreme points (terminals), the opposite electricities tend to annihilate each other; and if the fluid medium could be a substance incapable of decomposition, the equilibrium, there is every reason to believe, would be restored, and the motion of the electricity cease. But solution of muriate of soda being composed of two series of elements possessing opposite electrical energies, the oxygen and the acid are attracted by the zinc, and the hydrogen and the alkali by the copper. The balance of power is momentary only; for solution of zinc is formed, and the hydrogen disengaged. The negative energy of the copper and the positive energy of the zinc are consequently again exerted, enfeebled only by the opposing energy of the soda in contact with the copper, and the process of electromotion continues, as long as the chemical changes are capable of being carried on."

Davy concluded the lecture with comments on the operation of electrolysis in nature. He described the result of electrolizing a piece of muscle fibre, a laurel leaf and mint. He remarked: "When the fingers, after having been carefully washed with pure water, are brought in contact with this fluid in the positive part of the circuit, acid matter is rapidly developed." Here he seemed to have observed the emergence of acid ions from the body under the influence

of an electric current. He explained that the tastes produced on the tongue by two electric terminals of the opposite sign seemed to be due to the decomposition of the living animal substances, and the saliva. Then he says: "As acid and alkaline substances are capable of being separated from their combinations in living systems by electrical powers, there is every reason to believe that by converse methods they may be likewise introduced into the animal economy, or made to pass through the animal organs: and the same thing may be supposed of metallic oxides; and these ideas ought to lead to some new investigations in medicine and physiology."

They are now the basis of that branch of medical treatment named Ionic Medication. Uric acid is removed from the joints by sending currents through them into a water bath. The currents carry the uric acid ions out of the joint into the water. Iodine is carried into the body by similar currents. Superfluous hair is removed by decomposing the roots electrolytically.

In a footnote Davy remarked: "Seeds, I find, when placed in pure water in the positive part of the circuit, germinate much more rapidly than under common circumstances; but in the negative part of the circuit they do not germinate at all. Without supposing any peculiar effects from the different electricities, which however *may* operate, the phenomenon may be accounted for from the saturation of the water near the positive metallic surface with oxygen and of that near the negative surface with hydrogen."

He then remarked that electrolysis ought to provide effective methods for the manufacture of acids and alkalies. It should lead to the discovery of the *true* elements of substances, because the chemical affinities of elements, which depend on electrical affinities, are probably limited in degree, "whereas the powers of our artificial instruments seem capable of indefinite increase." This passage is of great interest in relation to recent experiments on the disintegration of atoms by powerful electrical machinery.

He explained that electrolysis must have had a most important rôle in the formation of geological strata; "where

pyritous strata and strata of coal-blende occur . . . electricity must be continually manifested."

This remark was the birth of the electrical method of prospecting for minerals. He concluded the lecture with the paragraph: "Its slow and silent operations in every part of the surface will probably be found more immediately and importantly connected with the order and economy of nature; and investigations on this subject can hardly fail to enlighten our philosophical systems of the earth; and may possibly place new powers within our reach."

The publication of Davy's first Bakerian Lecture established permanently his status as a great scientist. His views were so modern that they now seem to belong more to the twentieth than to the nineteenth century. His famous competitor in the development of electro-chemistry was Berzelius, who entered the subject soon after him, and afterwards in some directions developed it further. No one could have considered Davy's contribution more critically, and he said that it was one of the most remarkable of all contributions to the theory of chemistry. The impression made by Davy's lecture was such that the Institute of France awarded him a prize of three thousand francs, though England and France were at war. This intelligent and courageous act drew abuse on the Institute of France, and abuse on Davy in England. Concerning this matter, he wrote to Poole: "Some people say I ought not to accept this prize; and there have been foolish paragraphs in the papers to that effect, but if the two countries or governments are at war, the men of science are not. That would, indeed, be a civil war of the worst description; we should rather, through the instrumentality of men of science, soften the asperities of national hostility."

In 1807, when he was twenty-eight years old, he was elected one of the three secretaries of the Royal Society. Wollaston was the other ordinary secretary, and Young was the foreign secretary. The foreign membership of the Institute of France was restricted to eight persons, and the three secretaries of the Royal Society were included among

the eight. It is a mark of the magnitude of their international reputation. Davy was devoted to the affairs of the Royal Society. He was an active secretary, and many papers were communicated to the *Transactions* through him.

During 1807 he continued his electro-chemical researches. In his first Bakerian Lecture he had plainly expressed his expectation that no chemical compound would be able to withstand the decomposing effects of electrical machines of sufficient power. As early as 1800 he had followed Henry in the attempt to decompose potash by electricity, but found that the solution of potash was merely made stronger at one of the poles by the action of the current. In 1807 he returned to the problem of the composition of potash and soda. He suspected that these substances could not be elements, partly by analogy with ammonia. This volatile alkali contained hydrogen and nitrogen. Perhaps in the denser alkalies, such as potash, the hydrogen might be replaced by a denser substance, such as phosphorus or sulphur. No combinations of these substances with nitrogen were known, so "it was probable that there might be unknown combinations." The later chemists of the nineteenth century were shocked by this train of deduction, but it was splendidly sufficient for its purpose. The possible existence in potash of an element heavier than hydrogen was Davy's significant and sufficient inspiration.

Another inspiration of Davy's, which seemed mistaken to the nineteenth-century chemists, was his profound conviction that nitrogen was not an elementary substance. He spent much time on attempts to decompose it, necessarily without success. But in the deepest view his inspiration is now seen to be correct, for the nitrogen atom was the first to be disintegrated by Rutherford and his successors in our own time. Davy rightly suspected that a substance which so markedly lacked clear-cut chemical properties could not be simple, or as we should say now, could not be symmetrical and hence strong in structure.

Davy's power of looking directly at phenomena as they are, and not as they appear through the spectacles of theory,

is finely illustrated by that incident in the history of chemistry. In 1799, when deprecating too much his own early theoretical speculations, he wrote: "One good experiment is worth more than the ingenuity of a brain like Newton's." When he was twenty-eight Davy justified, as far as it is possible to justify, the bold statement he had made when he was twenty-one. In repeating the effect of electrolysis on solutions of potash and soda he noted again that apart from the evolution of hydrogen and oxygen no other important chemical effect occurred. Hence "the presence of water appearing thus to prevent any decomposition, I used potash in igneous fusion. . . . Though potash, perfectly dried by ignition, is a non-conductor, yet it is rendered a conductor by a very slight addition of moisture, which does not perceptibly destroy its aggregation; and in this state it readily fuses and decomposes by strong electrical powers. . . . A small piece of pure potash, which had been exposed for a few seconds to the atmosphere, so as to give conducting power to the surface (was attached to electric terminals in the open air). Under these circumstances a vivid action was soon observed to take place. The potash began to fuse at both its points of electrization. There was a violent effervescence at the upper surface; at the lower, or negative surface, there was no liberation of elastic fluid; but small globules having a high metallic lustre, and being precisely similar in visible characters to quicksilver, appeared, some of which burnt with explosion and bright flame as soon as they were formed, and others remained and were merely tarnished, and finally covered by a white film which formed on their surfaces."

Davy discovered potassium and sodium in the early part of October, 1807. By November 19th he had elucidated many of their properties, and delivered his second Bakerian Lecture on that date. The intervening days were spent in the wildest intellectual excitement and experiment. When Davy first "saw the minute globules of potassium burst through the crust of potash and take fire as they entered the atmosphere he could not contain his joy—he actually bounded about the room in ecstatic delight, and that some

little time was required for him to compose himself sufficiently to continue the experiment."

He soon found that the globules could be preserved most conveniently in naphtha. The base of potash, which he named potassium (spelt potasium in his lecture), appeared to melt at about 50° F. and to boil at a red heat. It was brittle at the freezing point of water, but soft and malleable at room temperature. It was a perfect conductor of electricity and heat. Its specific gravity appeared to be about 0·8. These physical properties of the new metal were sufficiently surprizing, but the chemical properties proved even more surprizing. It burned in chlorine gas with a bright red light. "When a globule of the basis of potash is placed upon ice it instantly burns with a bright flame, and a deep hole is made in the ice, which is found to contain a solution of potash." . . . "So strong is the attraction of the basis of potash for oxygen, and so great the energy of its action upon water, that it discovers and decomposes the small quantities of water contained in alcohol and ether, even when they are carefully purified." He gave a summary of the reactions of potassium with the mineral acids, phosphorus and sulphur. He described the formation of liquid amalgams with mercury, the reduction of metallic oxides when heated in contact with potassium and the decomposition of glass. The discovery of boron, which appeared as a dark-coloured inflammable substance during the decomposition of boric acid, was included among the details. Similar technique showed that the basis of soda, which he named sodium, was a soft metal of specific gravity about ·93. Its melting point was higher than that of potassium, and its rate of combination with oxygen was less rapid. It showed a similar series of reactions with sulphur, mercury, etc. He then described quantitative experiments to elucidate the proportions of the constituents in the oxides of potassium and sodium. He operated with very small quantities and obtained fairly accurate results.

Having discovered oxygen in the alkalies, he then convinced himself by several experiments that oxygen was contained in ammonia, which was correctly supposed to

consist of hydrogen and nitrogen. He was impelled into this error by his nationalistic desire to prove spectacularly that oxygen was the principle of alkalinity in opposition to Lavoisier's theory that it was the principle of acidity. This is a curious example of the influence of economic and political conflicts on the development of scientific thought. Davy's important work on chlorine reflected the same motives. The development of science clearly reflects the struggle between England and France and the deeper sociological conflicts underlying this struggle. In the concluding remarks of the Lecture he remarked that potassium and sodium "will undoubtedly prove powerful agents of analysis; and having an affinity for oxygen stronger than any other known substances, they may possibly supersede the application of electricity to some of the uncompounded bodies."

Davy's first Bakerian Lecture made a strong impression on the learned world, but the second made an enormous impression. The new substances he had discovered were themselves so exciting. The general opinion of posterity is that while the second was more spectacular, the first was more fundamental. In assessing the importance of scientific discoveries the value of the spectacular aspect should not be underrated. Very striking new facts can have a powerful stimulus for the imagination, and attract workers to fields they would otherwise have passed.

While Davy was experimenting and writing in the extremest excitement, during the days before November 19th, he became feverish. He redoubled his exertions from the fear that he might die before his descriptions were finished. After his Lecture was delivered he collapsed into a serious illness and took to his bed on November 23rd. Its nature remained obscure, though he believed afterwards it was typhus fever, caught during a visit to Newgate prison, in order to suggest methods of disinfection. His doctors considered it was due to over-fatigue and excitement. In the last years of his short life be became again the victim of obscure nervous disease. These events suggest speculations concerning his physiological constitution. His exceptionally high pulse-rate, and general behaviour may

have been due to some abnormal activity of the thyroid or other glands. His early maturity and early death suggest that the physiological processes of his body worked at an abnormally high speed. If that is so, his early death was a natural consequence of his early maturity, and, from the prospect of science, cannot be reasonably regretted, for his physiological constitution would have prevented him from continuing to work in his elder years as he worked in his earlier years. Though he died at the age of fifty-one he did not die prematurely; he had merely lived swiftly. Perhaps his early death is to be ascribed to other causes which will be discussed later in this chapter. The parallels of the life of Balzac are interesting. Like Davy, he died at the age of fifty-one, nervously prostrate. In his early years he worked with similar extreme intensity, he had the same passion for glory and for patrician rank. He was devoted for years to the wife of a Polish count, and had conventional religious views. His artistic genius impelled him to create characters whose truth denied the accuracy of his own opinions, as Davy's scientific genius impelled him to create the science out of which manufacturers, whom he held in social contempt, made fortunes. Balzac was impelled to destroy the aristocracy he admired through the instrument of realistic literature, and Davy was impelled to destroy the aristocracy through his development of science capable of profitable application by another social class.

At the extreme height of his fame Davy lay near death. Bulletins were issued, similar to those of princes, and his illness even increased his popularity. Eminent specialists refused to accept fees for attending him. After nine weeks he entered convalescence, but he was unable to leave his bed because he did not possess a sofa. The Royal Institution bought one for him for three guineas. The cessation of his lectures reduced the Institution's income from £4141 in 1807 to £1560 in 1808. Davy's salary from the Institution never exceeded £300 per ann. Before his marriage he showed no interest in the acquisition of wealth. His brother John resided in the Institution from 1808 to 1811, in the next room to Davy's, and has described the

simplicity of his furniture and the disorder. Complimentary letters from eminent persons, from scientists of international fame, were thrown together into a cupboard, with anonymous sonnets from young ladies. His quarters were only a place for sleep and hasty changes between the laboratory and evening parties. He was adored by Mrs. Greenwood, the housekeeper of the Institution, who nursed him almost continuously during his illness. While he was convalescing, public sympathy was converted into subscriptions for the construction of large Voltaic batteries.

The French chemists appreciated and developed Davy's discoveries as swiftly as he had developed Volta's. When he recovered, he was annoyed to find that they had accomplished a number of researches he had hoped to make. While he was attempting to extend his decompositions to baryta and lime, Berzelius informed him that he and Pontin had succeeded in obtaining barium and calcium amalgams. Davy immediately extended their method to the production of strontium and magnesium amalgams.

During the next two years Davy discovered a number of new substances and made determined attempts to decompose nitrogen. His Bakerian Lectures for 1808 and 1809 were first class, but did not contain fundamental discoveries equal in importance to those described in his earlier Bakerian Lectures. They displayed his weakness in systematic work, and his admirers were disappointed. The Royal Society had acquired the habit of inviting him to give their chief annual lecture on physical science, so he had to make it an interim report on his researches when there was no fundamental discovery to announce.

When the large battery, built from the proceeds of the public subscription, was completed the first experiments produced remarkable effects. If two pieces of charcoal connected to the terminals of the battery, and about an inch long and one-sixth in diameter, were brought nearly in contact, within one-fortieth of an inch, a bright spark appeared in the intervening space. The ends of the charcoal became white-hot, and when the pieces were withdrawn a constant discharge occurred through a distance of

about four inches, which emitted a most brilliant light. This was the invention of the arc-light and electric furnace. Platinum, quartz, sapphires, magnesia and lime were fused in the arc, and charcoal, plumbago and diamond were observed to disappear. Davy wrote: "I have kept charcoal white-hot by the Voltaic apparatus, in dry oxymuriatic gas for an hour, without effecting its decomposition. This agrees with what I had before observed with a red heat. It is as difficult to decompose as nitrogen, except when all its elements can be made to enter into new combinations."

This observation was his chief scientific inspiration in his attack on Lavoisier's conception of oxymuriatic acid gas. This name was proposed by Lavoisier for the gas that Sheele obtained from pyrolusite by heating it with marine acid (hydrochloric acid). He named the gas dephlogisticated marine acid, i.e. marine acid freed from hydrogen, and discovered several of its characteristic properties. Lavoisier named the gas according to his own theory and nomenclature, which required all acids to contain oxygen. By the second decade of the nineteenth century the sound parts of Lavoisier's great theory had been thoroughly assimilated and the weak parts were beginning to be a nuisance. Davy's chemical insight and rivalry with the French urged him to suspect that oxymuriatic acid gas contained no oxygen, and that the French theory was defective. When he had satisfactorily proved his opinion he renamed the gas *chlorine* from its colour.

Davy accurately defined a chemical element as a substance that could not be decomposed by any known chemical process. He showed that chlorine was an element in this sense and that it did not contain oxygen. This research is considered by chemists to be his finest exhibition of chemical technique. In his day moisture could be excluded from chemical experiments only with great difficulty. Nearly all substances contained some moisture, even if they did not contain combined oxygen. Hence some oxygen could nearly always be obtained from the water of the moisture, even if the substances contained no combined oxygen.

Davy repeated Cruickshank's experiment of exploding a mixture of chlorine and hydrogen. He dried the gases, but was never "able to avoid a slight condensation; though in proportion as the gases were free from oxygen or water this condensation diminished."

Henry had shown by electrolysis that hydrogen could be obtained from hydrochloric acid gas, but he assumed it came from water in the gas.

At the time, the liquor of Libavius (tin chloride) was supposed to be a combination of tin oxide and hydrochloric acid. On that assumption Davy tried to obtain tin oxide from it by combining the hydrochloric acid part with ammonia. He obtained a white solid that was not tin oxide.

He tried to obtain oxygen from phosphorus pentachloride, which was supposed to be a compound of the oxygen-containing phosphoric acid and hydrochloric acid. But the treatment of phosphorus pentachloride with ammonia did not appear to remove hydrochloric acid from it, with the formation of volatile sal ammoniac. To his great surprise he obtained a substance stable at a high temperature.

He found that if potassium is heated in chlorine gas a dry substance was formed. If potassium oxide was substituted for potassium the same substance was obtained and the oxygen was released. He argued that "it is contrary to sound logic to say that this exact quantity of oxygen is given off from a body not known to be compound when we are certain of its existence in another." Again, if "the oxygen arises from the decomposition of the oxymuriatic gas, and not from the oxides, it may be asked why it is always the quantity contained in the oxide." Davy continues: "I have made an experiment which seems to prove that the pure gas is incapable of altering vegetable colours, and that its operation in bleaching depends entirely upon its property of decomposing water and liberating its oxygen."

He discussed the combustion of various metals in chlorine, commenting that it was more violent in some cases than in oxygen. As the heat and light of combustion is due only to the violence of the reaction he did not

consider that the emission of heat and light in a chemical reaction was a necessary sign of the presence of oxygen.

He attempted to decompose chlorine by electric spark discharges, but without success. In his experiment with the carbon arc in chlorine he had exposed the gas to high temperature and also white-hot carbon. If oxygen were in the chlorine it would have been drawn out by the hot carbon.

Davy's variety of experiments, exactitude and acute reasoning were seen in their finest development in his chlorine researches. After he had shown that chlorine did not contain oxygen he proved that chlorine and oxygen can combine in a substance he named euchlorine. The presence of this substance in some experiments made by his critics had helped to continue the confusion, and his elucidation of this cause of confusion added a convincing confirmation to his thesis of the elementary nature of chlorine.

Davy was now thirty-three years old and at the summit of his scientific career. He had decided to marry Mrs. Apreece, a rich and fashionable widow, and retire from the professional direction of the Royal Institution.

His last course of lectures as the Royal Institution's professor was announced in 1812. A youth named Michael Faraday was given a ticket for the lectures by a Mr. Dance. Faraday carefully made notes of the lectures, which filled 386 pages of manuscript. This famous book still exists in the library of the Royal Institution. Faraday wrote to Davy near the end of 1912 explaining that he wished to escape from trade, which he thought vicious and selfish, and serve science, which he imagined made its pursuers amiable and liberal. He therefore asked for any laboratory appointment that might be available, and enclosed his notes of the lectures as evidence of his fitness.

Davy replied:

"*Decemb.* 24*th*, 1812

SIR,

I am far from displeased with the proof you have given me of your confidence, and which displays great zeal, power of memory and attention. I am obliged to go out

of town, and shall not be settled in town till the end of January. I will then see you at any time you wish.

It would gratify me to be of any service to you. I wish it may be in my power.

I am, Sir, your obedient humble
servant,
H. DAVY."

Early in 1813 he sent for Faraday and told him of the situation of assistant in the Laboratory, then vacant. He still advised him not to lose his prospects as a bookseller's apprentice, saying science was a harsh mistress, and smiled at his notion concerning the superior moral feelings of philosophic men, saying he would leave him the experience of a few years to set him right on that matter.

From the perspective of history this was the greatest action of Davy's personal life. It is impossible not to be moved by his generous feeling, profound insight and perfect behaviour on this occasion. Almost at the moment when he was helping with perfect understanding a youth of scientific genius and social origin similar to his own, he was committing the chief error of his personal life through failure to understand himself and a member of another social class. Until he was married in April, 1812, he had lived in simple bachelor rooms in the Royal Institution. His total official salary was £400 per ann. Though he had been passionately fond of social pleasures he had always subordinated them to scientific work. On April 8th he was knighted, and on the 11th he married Mrs. Apreece. He did not intend his marriage to interfere with his prosecution of research. He relinquished his official appointment in order to be free from administrative work, but he remained honorary professor, and hoped to continue to devote much time to research.

Mrs. Apreece was the widow of S. A. Apreece, the eldest son of Sir Thomas Apreece. She was the daughter of Charles Kerr, a distant relative of Sir Walter Scott, and a merchant who had made a large fortune in Antigua, a West Indian island used for sugar-growing and slavery. The

origin of Mrs. Apreece's wealth may have stimulated her to pursue the respectability of aristocratic rank. Her passion for rank was as intense as Davy's, but she had no genius, though she strenuously cultivated her talents. She was a friend of Mme. de Staël and the social lioness of Edinburgh, when the society of the city was particularly provincial. At the time of her second marriage G. Ticknor wrote that "she is small, with black eyes and hair, a very pleasant face, an uncommonly sweet smile and, when she speaks, has much spirit and expression in her countenance. Her conversation is agreeable, particularly in the choice and variety of her phraseology, and has more the air of eloquence than I have ever heard before from a lady. But then, it has something the appearance of formality and display, which injures conversation."

The two social hunters allied in the attack on the aristocratic stockade. In his early letters to his mother concerning his marriage, Davy referred to his wife as "Lady Davy." The pursuit of science was rapidly subordinated to the pursuit of snobbery. He withdrew from a company formed to manufacture gun-powder because he had "resolved to make no profit of any thing connected with science." He saw no paradox in devoting his "life to the public" at the expense of the fortune inherited from the merchant of Antigua.

In 1813 Napoleon gave him permission to visit the volcanoes at Auvergne, though France and England were at war. Davy went to France with his wife, and Faraday as his "assistant in experiments and writing." He behaved with a remarkable mixture of arrogance and courage. He refused to sit through the performance of a play that included satire on the English, and for a long time refused to accept an invitation to call on the Empress in other than the sort of dress in which he was knighted. He was not polite to the distinguished French chemists that assembled to meet him in Paris, though they received him with an honour extraordinary in a time of war. Lady Davy persisted in treating Faraday as a personal servant, and Davy did not seem to know how to manage the delicate situation

between his newly-wedded wife and his young scientist. Davy took a small box of chemical apparatus in order to conduct experiments during his tour. He also worked in Paris laboratories. While in France he heard of the discovery of a substance which becomes a violet-coloured gas when heated. He seized the clue and worked out the properties of the substance, and named it iodine, showing that it was an element similar to chlorine. This achievement with the equipment of his travelling-box of apparatus was technically remarkable, but made much ill-feeling as the French chemists considered he had stolen a theme which they had discovered and which they were examining.

Davy continued his tour in Italy, and visited Volta, who was astonished by his shabby appearance. Throughout his travels Davy, consciously or unconsciously, extracted the maximum latitude allowed him in virtue of his scientific eminence. As an observer has reported, he sometimes behaved like a coxcomb, but his courageous assertion of the privileges of a distinguished scientist, even if inspired more by social ambition than respect for science, contributed towards the establishment of an increased public estimation of the importance of the scientific contribution to culture. Davy helped to create the social prestige of the modern scientist. He received only one honorary degree from a university, but his successor Faraday received many.

After his return to England Davy spent more and more time in country house parties. He thoroughly enjoyed shooting and fishing. He visited his wife's relative, Walter Scott, and strained to establish his position as an aristocrat. Within three years his marriage had proved a conspicuous failure. His wife had no children, and had no interest in domestic life. Davy's brother wrote that Lady Davy was "fitted to excite admiration rather than love, and neither by nature happy in herself or qualified to impart, in the best sense of the term, happiness to others." "There was an oversight, if not a delusion, as to the fitness of their union," and "it might have been better for both if they had never met." While describing a party he attended in 1838 Ticknor writes: "The aristocracy and fashionable (were

represented) by the haggard, dried-up Lady Davy." Sometime after he had married, Davy was asked for advice by a relative contemplating marriage. He wrote: "Upon points of affection it is only for the parties themselves to form just opinions of what is really necessary to ensure the felicity of the marriage state. Riches appear to me not at all necessary, but competence I think is; and after this more depends on the *temper* of the individual than upon personal, or even intellectual circumstances. The finest spirits, the most exquisite wines, the nectars and ambrosias of modern tables will be all spoilt by a few drops of bitter extract; and a bad temper has the same effect in life, which is made up, not of great sacrifices or duties, but of little things in which smiles and kindness and small obligations given habitually are what win and preserve the heart and secure comfort."

It is natural to conclude that Davy was writing from his own experience.

While his domestic happiness failed, his great public reputation was enormously extended by his invention of the miner's safety-lamp. In 1812 ninety-two men and boys had been killed in an explosion in the Felling Mine near Gateshead-on-Tyne. This and other explosions prompted J. J. Wilkinson to form a society for the study and prevention of mine explosions. Wilkinson called at the Royal Institution in the autumn of 1813 to invite Davy's co-operation, but he was then absent in Paris. When he returned, Dr. Gray, the rector of Bishopwearmouth, repeated the invitation, and Davy replied:

'*To the Reverend Dr. Gray.*

August 3rd, 1815.

SIR,

I had the honour of receiving the letter which you addressed to me in London at this place, and I am much obliged to you for calling my attention to so important a subject.

It will give me great satisfaction if my chemical knowledge can be of any use in an inquiry so interesting to

humanity, and I beg you will assure the Committee of my readiness to co-operate with them in any experiments or investigations on the subject.

If you think my visiting the mines can be of any use I will cheerfully do so.

There appear to me to be several modes of destroying the firedamp without danger; but the difficulty is to ascertain when it is present without introducing lights which may inflame it. I have thought of two species of lights which have no power of inflaming the gas which is the cause of the firedamp, but I have not here the means of ascertaining whether they will be sufficiently luminous to enable the workmen to carry on their business. They can be easily procured, and at a cheaper rate than candles.

I do not recollect anything of Mr. Ryan's plan: it is possible that it has been mentioned to me in general conversation, and that I have forgotten it. If it has been communicated to me in any other way it has made no impression on my memory.

I shall be here ten days longer, and on my return south will visit any place you will be kind enough to point out to me where I may be able to acquire information on the subject of the coal-gas.

Should the Bishop of Durham be at Auckland, I shall pay my respects to his Lordship on my return.

I have the honour to be, dear Sir, with much respect,

Your obedient, humble servant,

H. Davy.

At Lord Somerville's,

near Melrose, N.B."

This is Davy's second great letter. As in his reply to Faraday, he showed his remarkable gift for recognizing and attending to important new matters. His willingness to consider novel suggestions was one of his finest qualities.

On his journey to the south he visited the Gateshead district and made some preliminary investigations in the company of the Rev. Mr. Hodgson, who had preached the

sermon at the burial of the Felling victims, and whose account of the disaster had been widely read. After his return to London Davy received from Hodgson some specimens of firedamp, the explosive gas found in coal mines, and began an experimental investigation of its properties. Within two or three weeks he had discovered "that explosive mixtures of mine-damp will not pass through small apertures or tubes, and that if the lamp or lanthorn be made airtight on the sides, and furnished with apertures to admit the air, it will not communicate flame to the outward atmosphere." He found afterwards that Wollaston and Tennant had already discovered that mixtures of air with the gas distilled from coal would not explode in very small tubes. But Davy investigated the phenomenon in detail. He found that "explosions were stopped by metallic tubes of one-fifth of an inch when they were an inch and a half long; and this phenomenon probably depends upon the heat lost during the explosion in contact with so great a cooling surface, which brings the temperature of the first portions exploded below that required for the firing of the other portions."

As in his best researches, Davy described his discoveries with remarkable elegance. The combination of novel facts with powerful description easily explains the profound impression made by Davy on his contemporaries.

After determining for himself the behaviour of fire-damp in fine tubes, he showed that explosions would not pass through wire gauze. The recognition that wire gauze consists of a very large number of very short fine parallel tubes is an excellent example of his power of scientific generalization. By January 11th, 1816, after three months of research, he had devised the safe-lamp in which the flame is surrounded by wire gauze, through which the fire-damp can pass from the outer atmosphere to the flame. He had shown that the fire-damp could burn inside the gauze-cylinder with a bright light and raise the gauze to a red heat without causing an explosion in the inflammable atmosphere surrounding the lamp. Thus his lamp would burn safely in an inflammable atmosphere of air and fire-damp, and

also reveal the presence of fire-damp. Incidentally, he made important contributions to the study of flame. This is yet another example of the power and rapidity of his genius. And how did he phrase his view of the social importance of his researches on the safety-lamp? His opening paragraphs in the collected papers on Flame, published in 1818, are:

"The use of pit-coal in Britain is connected not only with the necessaries, comforts and enjoyments of life, but also with the extension of our most important arts, our manufactures, commerce and national riches.

Essential in affording warmth and preparing food, it yields a sort of artificial sunshine, and in some measure compensates for the disadvantages of our climate. By means of it metallurgical processes are carried on, and the most important materials of civilized life furnished, the agriculturist is supplied with a useful manure, and the architect with a necessary cement. Not only manufactories and private houses, but even whole streets are lighted by its application, and, in furnishing the element of activity in the steam-engine, it has given a wonderful impulse to mechanical and chemical ingenuity, diminished to a great extent human labour, and increased, in a high degree, the strength and wealth of the country.

Everything connected with the permanent supply of such a material is worthy of scientific consideration, and to remove obstacles, difficulties or dangers connected with its production is not unimportant to the State."

Another manifesto of the applied scientist! The direction of British history was fundamentally affected by two mighty events of 1815: Wellington's victory over Napoleon and Davy's victory over fire-damp. Davy's victory paid for Wellington's.

The safety-lamp allowed the coal industry to grow rapidly. It did not diminish the number of miners killed because it greatly increased the number exposed to danger by making deeper and larger mines workable. Davy refused to patent the invention because his "sole object was to serve the cause of humanity." The chief effect of his

invention was to increase the wealth of the owners and bring more men into the mines and expose them to the dangers of which fire-damp is one only. Hence Davy's lamp was more important as an instrument of economics than of safety.

Davy was fêted and applauded in Britain and Europe. He reiterated his magnanimous sentiments concerning his sole interest in the cause of humanity, but he became more than angry at the assertions of the friends of George Stephenson, who said that Stephenson deserved the priority in the invention. The famous railway engineer was then an obscure wheelwright at Killingworth Colliery. He was independently working towards a safety-lamp designed with fine holes for admitting the inflammable mixture of fire-damp and air. His mechanical genius would probably have enabled him to produce a perfected safety-lamp by persistent trial and alteration. But he was groping towards the solution, whereas Davy had swiftly analysed the problem and solved it thoroughly.

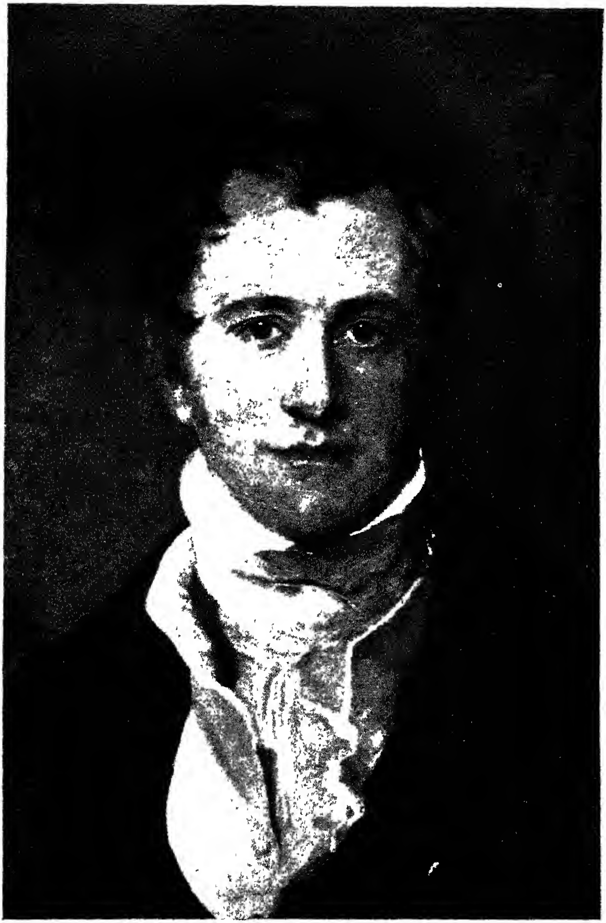
On September 25th, 1817, Davy was presented with a valuable set of plate by a committee of coal-owners. In his speech he referred to "certain calumnious insinuations which have arisen, not in the scientific world, for to that the whole progress of my researches is well known, but in a colliery. I must ever treat these insinuations with contempt . . . I could never have expected that such persons would have engaged their respectable connexions in mean attempts to impeach the originality of a discovery. . . . I do not envy them their feelings, particularly at the present moment: I do not wish to inquire into their motives: I do hope, however, that their conduct has been prompted by ignorance rather than by malevolence, by misapprehension rather than by ingratitude."

While Davy was being presented with plate, Stephenson was being given a present by his admirers. Davy's sentiments concerning his sole interest in humanity were soiled by his vituperation against honest George Stephenson. In argument, if not in behaviour, the Davy party was, on the whole, in the right, but there is little doubt that without

Davy Stephenson would in the end have produced a satisfactory lamp. Apart from the exposure of an aspect of Davy's character, the incident is instructive because it shows that the pressure of industrial necessity was forcing a solution of the problem of the safety-lamp along more than one line. After 1815 Davy accomplished no great researches. He became more aristocratical, and again toured Britain and Europe. In 1820 he succeeded Sir Joseph Banks as President of the Royal Society. Banks had been opposed to his nomination as he preferred Wollaston, whose manner was more sedate. But Wollaston withdrew, so Davy received the honour he had so deeply coveted. In spite of his extreme desire to be a great president, Davy was not exceptionally successful. He mismanaged his social entertainments and fretted at the refusal of the governing class to allow more prestige to science and himself. He was hasty in the conduct of business affairs. He had not at first supported the nomination of Faraday for a Fellowship. Nevertheless, in spite of his temperamental weaknesses, Davy did not decrease the eminence of the presidency. His genius and fame were too great. His loves for science and for himself were both very strong, and equally real. The first enabled him to accomplish several very important administrative contributions to culture. He had a leading part in the founding of the British Museum of Natural History, the Zoological Gardens and the Athenæum Club.

Davy's unhappy married life probably hampered him in the performance of his social duties. It probably also sent him on the almost continuous country visiting and travels. He was irritated by the failure of investigations with which he had been entrusted by the Crown, such as the unrolling of manuscripts from Herculaneum and the prevention of the corrosion of the copper sheathing of battleships. He was willing to undertake anything, and was unreasonably angry when he was not always successful.

In 1825 he became ill. He began to have symptoms of paralysis. He became very irritable, and went on a tour to Italy without his wife. From 1825 until 1829, when he



SIR HUMPHRY DAVY

(*From a portrait by Sir Thomas Lawrence in the possession of the Royal Society*)

died, he filled his copious diaries with long descriptions of natural scenery and fauna. His letters to his wife are in affectionate but very restrained phraseology, as if he were severely disciplining himself to be reasonable. The student must reflect on the cause of his paralysis. Was it a premature exhaustion of the nervous system by an excessively active life, perhaps connected with glandular abnormality? Or had he contracted venereal disease in his youth? His temperament was ardent and experimental. If he had, an additional explanation of the collapse of his married life and childlessness would be found. In his biographies there is no suggestion that he had such a disease.

After four years of helpless wandering he died at Geneva on May 29th, 1829. His brother John, who had become a distinguished naval surgeon, was with him and wrote: "Respecting the nature of the complaint and the immediate cause of the death of my dear brother I have nothing to state that is at all satisfactory to myself." Davy had said that there was to be no *post mortem* examination, so the "exact kind and immediate cause of his death must ever remain doubtful."

Davy died at the age of fifty-one years. In his methods of work he was a brilliant opportunist and exploiter, like the nineteenth-century industrialists that followed him and of whom he was the premonitory intellectual shadow. He was dependent on the suggestive originality of Beddoes and the inventive originality of Volta, for he could not work in intellectual fields not close to immediate interests. He exploited fields that others had made promising by thought or invention with immense energy and brilliance. He was temperamentally related to the capitalists, and in the latter part of his life tried to capitalize his scientific eminence into social rank, as his wife tried to secure social rank through wealth accumulated by her Scottish father in Antigua. His copious writings were florid and without talent, except when their subject was science. Then sometimes the rhetoric was filled with profound and new truth. Many of Davy's descriptions of chemical phenomena still appear

fresh, exact and final. If they can so impress the mind after more than a hundred years, how must they have impressed those that heard them for the first time? Davy's electrical theory of chemistry remains the basis of the modern conception of chemistry. How brilliant and weirdly new it must have appeared to his audiences in the Royal Institution and at the Royal Society! The new views were so clearly and enthusiastically expressed.

Davy showed the way to the scientific industrial society of the nineteenth century. Like the hitherto unparalleled achievements of that society, his genius was immense and often vulgar. He pursued science and snobbery with equal vehemence. He was the first man to explain adequately the importance of science to society, and he began the task of systematically introducing scientific method into all forms of industry. Humanity was fortunate to have such a guide at one of the most important periods of its history.

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II

MICHAEL FARADAY

1791–1867

II

MICHAEL FARADAY

1791–1867

FARADAY was the greatest physicist of the nineteenth century and the greatest of all experimental investigators of physical nature. He is a member of the small class of supreme scientists, which includes Archimedes, Galileo, Newton, Lavoisier and Darwin. Einstein has said that the history of physical science contains two couples of equal magnitude: Galileo and Newton, and Faraday and Clerk Maxwell. This is not one of the less interesting of Einstein's equations. From it one can deduce an instructive result. No one would allow that even the wonderful Clerk Maxwell was as great a scientist as Newton. If the Faraday–Maxwell couple is to equal the Galileo–Newton couple Faraday must be accounted a greater scientist than Galileo. This deduction indicates his place in the history of science.

Faraday's contributions to science were even greater than Davy's, but his scientific superiority was gained at the expense of a limitation of cultural spirit. The struggle for the establishment of science as an integral part of civilized production and culture was conducted and won by his brilliant predecessor, in so far as such struggles are conducted by one man. Davy had established the social necessity for science, and Faraday proceeded along a route whose importance was accepted. He followed the route with extreme concentration, and jettisoned by accident and plan all mental furniture and social habits that might impede his progress. He was more like an athlete than a

philosopher, and he trained rather than educated himself. No athlete ever trained himself with more detailed determination than Faraday, or with more success. His imaginative and manipulative gifts were reinforced by ferocious perseverance, and their highest development was not accomplished until he was over forty years old. At forty he discovered how to obtain electric currents from magnetic fields. At fifty-four he discovered how light may be affected by magnetism. At about the same age he was conceiving those ideas of the nature of the field of electric force which have provided the foundation of modern electrical theory, and of the theory of relativity. At the age of sixty-nine he was speculating on the relations between electricity and gravity, and groping towards the conception of their interaction as experimentally proved in 1919, when electro-magnetic waves of light were observed to be distorted by the sun's gravity. At the age of seventy-one, in his last work he was searching for changes in the refrangibility of light effected by a magnetic field, an extremely important experiment not made with success until thirty-five years later, by Zeeman in 1897.

Faraday's powers grew slowly. He did not publish his first slight scientific research until he was twenty-five, and no important discovery until he was over thirty. His conceptual imagination did not become powerfully creative until he was about forty-five. This late development is unusual in men of supreme genius. Newton reached his conceptions of gravitation and the calculus before he was twenty-four. Darwin recognized the evidence for biological evolution when he was twenty-eight. Lavoisier superseded the phlogiston theory before he was thirty. Einstein published his first theory of relativity when he was twenty-five.

The growth of Faraday's imaginative power in his middle and later years shows he must have possessed very large intellectual energy. His thought was perfected through the relentless pressure behind it. The head of his intellectual energy was raised high by psychological barriers erected in infancy. A lake of energy was conserved behind these

barriers, and lasted for an exceptionally long time. It did not begin to overflow freely until he was forty.

Intellectual energy was Faraday's most fundamental gift. A variety of circumstances enabled him to use this energy with an efficiency which, for the accomplishment of certain objects, was extreme. Faraday achieved supreme greatness in certain directions by cutting off expenditure of energy in all other directions, unconsciously and consciously. His natural and cultivated habits of self-control deceived many persons, and gave a false impression of mildness. He was inwardly fiery, as his most intimate friends knew. He refused to expend his fire on most of the chief human activities. He renounced thought on the problem of human destiny. He renounced the accumulation of wealth. He renounced parenthood by the accident of nature or by design. He renounced social life more and more as he grew older. He refused the Presidency of the Royal Society. Negation was a very important factor in Faraday's life. Much has been written about the beauty of his life and character, which have often been recommended as models. But negation is not admirable, though it may be necessary. Renunciation is the manœuvre by which human weakness avoids some of the failures to which it is exposed. Its nobility is much exaggerated. The man who attempts the achievement of a complete personality is often nobler than the man who achieves a limited personality by renunciation. In the inevitable comparisons of Davy and Faraday, Davy's positive weaknesses are nearly always held against him, while no comment is made on the weaknesses of Faraday's negative virtues. In spite of his vanity, snobbery and jealousy, Davy's human personality was larger than Faraday's. Some of the popular affection attracted by Faraday was due to his lack of expression on controversial matters. It was difficult to quarrel with him because he would not discuss general questions. Tyndall has spoken of him as "the great man-child," and probably did not reflect that this was not a compliment to Faraday as a mature human personality. Men should be men. Faraday's weaknesses were as extensive as Davy's, and less sympathetic. Faraday

avoided vanity, snobbery and jealousy partly by retreating from various regions of human activity, whereas Davy was afflicted by them because he entered these regions. We can admire Faraday's caution, but we can sympathize with Davy's gallant failures to find positive solutions to problems in the life of a complete human personality.

Like all other great figures in history, Faraday had the qualities through which the tendencies of his time found expression. His family were craftsmen, his father being a blacksmith. He belonged to a social class possessing creative initiative at the beginning of the industrial revolution. George Stephenson was a classical example of a member of this class. Like the majority of the leaders of capitalist development, Faraday was a Nonconformist. He was Nonconformist almost to the point of caricature, as he was a member of the small sect of Sandemanians. Again, like the nineteenth-century manufacturers he believed in individual competition; and again, in his work he was individualistic almost to the point of caricature. He never had a research assistant, and created no school. Though his inability to collaborate was inherent in his personality and habits, he sought conscientiously for a genius to continue the direction of the researches at the Royal Institution. His individualism was powerfully reinforced by the exclusiveness of his sectarian views. His sect was very small in membership, and its strictness contracted the circle of possible intimates, for one could not be both Sandemanian and social. Thus Faraday could ignore the social life and conserve his energy for research. Professor R. H. Tawney and other social historians have discussed the relations between religion and the rise of capitalism, and have shown how the mentality of the Puritan was consonant with the saving habits of the embryo capitalist. In Faraday one notices the connection between the dissenting mind, and the development of experimental inquiry. One wonders, too, how much his discoveries were due to class-difference. Though he trained himself remarkably, he was never assimilated by the leisured educated class; in this he differed from Davy, who also was the son of a craftsman,

MICHAEL FARADAY

Sir R. Hadfield and Messrs. Chapman & Hall)

but subordinated research to social distractions when assimilated by the leisured class. Faraday's permanent acceptance of class distinction probably reinforced his tendency to individual research. In 1831, when he began the most brilliant period in his career, he decided to drop all commercial consulting work. He could have earned £5000 a year in fees, but he renounced the prospect of wealth so that he would not be distracted from pure research. Though his salary from the Royal Institution was at that time £100 per ann., his total income from extra lecturing and non-commercial fees was about £500 to £1000 a year. He gave a large part of this to charity. He had no children and his wife accepted the simple style of living he desired.

He published most of his researches in the journal of the Royal Society, but he avoided the government of the Society. For years he rarely went to a meeting, and in later life, when offered the Presidency, he declined it. He disapproved of the influence in the Society of persons of merely social eminence. He considered that membership of the Society should be restricted to scientists. He had no patience with academic politics. He had no particular ability for getting on with people. He would not have flourished if he had not worked in a purely individualistic institution where collaboration was unnecessary. Though he controlled his lively temper he was unable to conceal impatience with the scientific ineptitude of others. Occasionally he was incapable of appreciating the merits of others, in particular, of Sturgeon, that admirable ex-private soldier who had been impressed by the phenomena of thunderstorms while at artillery manœuvres in the field, and in the attempt to learn the philosophers' explanations of these phenomena began a self-education which led him to learn Latin, Greek, French, Italian and German, and to the invention of the electro-magnet.

Faraday was not interested in politics. He was a conventional Royalist, and accepted the social order without particular criticism. His lack of collective feeling increased the intensity of his individual life. The contemplation of its absorbed intensity often turns the attention of the student

from the consideration of its social insufficiency. Awareness of the limitations of Faraday's personality enables the student more correctly to assess him as an historic figure. When several of the qualities of Faraday's personality, which the nineteenth century uncritically regarded as virtues, are seen as limitations, and these limitations are seen to be the necessary price of his achievements; Faraday becomes a more interesting figure. He is rescued from the spiritual embalming performed by Victorianism, and the concealments of bourgeois superficiality.

Faraday was born near London on September 22nd, 1791. His father was a blacksmith who had recently migrated from Clapham, a village near Ingleborough in Yorkshire. His mother was the daughter of a farmer in that district, and said to be of Irish descent. In 1809, when Faraday was seventeen, his parents moved into a house in Weymouth Street, Portland Place. His father had long been an invalid, and died in the next year. Mrs. Faraday kept herself by taking in lodgers until her sons could support themselves and her. When Michael secured an adequate income he supported her entirely. She died in 1838, after enjoying the prospect of the fame her son had already acquired.

Faraday's father was a member of the Sandemanian sect. This sect was founded on the views of the Reverend John Glas, a Scottish Presbyterian divine. He was deposed by the Presbyterian Church Courts in 1728, because he held that the Church should not be subject to any covenant, but be governed only by the doctrines of Christ and His Apostles. He held that Christianity could never become the established religion of any nation without being perverted. Christ visited the world to inform humanity of the existence of an eternal life after death, and not to establish any sort of worldly power. The Bible was a repository of perfect truth, and man was not permitted to add anything, or take anything away from it. It was the sole and sufficient guide for each individual, under all circumstances. Faith in the divinity of Christ is the gift of God, and the evidence of this faith is obedience to Christ's commandments.

Glas's views were promulgated by his son-in-law, Robert Sandeman. Congregations which professed these views were formed in the Ingleborough district of Yorkshire, in London, and in some other places. The London congregation met in a small chapel at the end of Paul's Alley, Red Cross Street, a poor district. The Sandemanians did not proselytize. Their congregation included only about twenty to forty members. The non-proselytizing principle of the sect had a profound influence on Faraday and established or increased his inability to create a school of scientific research. The exclusiveness of the tiny sect also prevented him from learning habits of co-operation and government. Though the habits of non-proselytization handicapped Faraday in one way, it helped him in another. He had an extremely deep regard for scientific fact, and its power to convince without argument or advertisement. Sandemanianism may even have been an important factor in the creation of Faraday's existence. The families of the sect were closely intermarried. It is recorded that members excommunicated from the sect suffered much, because their social contact with the faithful members was broken, and as the remaining members were mainly relatives, the excommunicated were divided from their relatives. The production of Faraday by an in-bred community is of biological interest. The London Sandemanians included other distinguished persons; the engraver Cornelius Varley, and the water-colour painter George Barnard.

The Sandemanians followed simple Apostolic practices. On the Lord's day they broke bread together and took a common meal in a room next to the chapel, casting lots for their places. Before they celebrated the Lord's supper at the close of the afternoon service, they collected money for charity and expenses. They washed each other's feet "whenever it can be an act of kindness to a brother to do so." New members were admitted after a public confession of sin and profession of faith; and they received the kiss of charity. They considered the saving of money sinful. Faraday observed this tenet scrupulously. The non-advertising and non-saving principles of his creed isolated

him from the stream of capitalism. They had been adopted from the communist social philosophy of the early Christians and prevented Faraday from becoming a nineteenth-century Liberal. The Sandemanians had no priests. They were led by a number of elders, elected unanimously by the congregation. The elders presided in turn at the services. They were expected to accept the sects' tenets without question. There is very little evidence that Faraday ever had any difficulty in accepting the Sandemanian creed. He had attended their services since infancy. He became a full member of the sect a month after he had married Sarah Barnard, who was already admitted. He was then twenty-nine years old, and his wife twenty-one. In 1840, when he was forty-nine years old, world-famous and suffering from mental disorder from over-work, and probably from repression, he was elected an elder. He was much less effective as a preacher than as a scientific lecturer, as his manner became devoutly earnest, and he lost his lecturing vivacity. His sermons consisted of a series of rapid citations from the Bible. When he was not presiding he was regularly called to read the passages chosen from the Bible by the presiding elder. C. C. Walker wrote that "the perfection of the reading, with its clearness of pronunciation, its judicious emphasis, the rich musical voice, and the perfect charm of the reader, with his natural reverence, made it a delight to listen."

Elders were expected to attend every Sunday. Faraday usually broke his attendance at meetings such as those of the British Association, which starts on a Wednesday and continues until the following Wednesday, in order to return to London for the Sunday service. On one occasion he absented himself in order to obey the Queen's command to dine at Windsor. The congregation did not find his impenitence or his explanation acceptable, for he defended his action. His eldership and even his membership was suspended, though he continued to attend the services regularly. Presently he was readmitted, and in 1860, after many years, re-elected an elder. Faraday never discussed religion without invitation. He considered it was

concerned with an order of truth different and higher than natural truth, and said with great emphasis that his religion contained no philosophy.

Supplied by his creed with a direct answer to the perplexing problem of human destiny, with freedom from acquisitiveness and social ambition, and with a respect for Royalty, he wasted no energy on philosophical thought, on the acquisition of wealth, on the achievement of rank, or on opposition to social injustice. He was perfectly fitted to achieve those objects that can be attained by the application of an extremely intense but narrow stream of energy. From five until thirteen Faraday received some sort of schooling. He said that "my education was of the most ordinary description, consisting of little more than the rudiments of reading, writing, and arithmetic at a common day-school. My hours out of school were passed at home and in the streets." He used to play marbles in Spanish Place.

At the age of thirteen he became errand-boy to a bookseller and newsagent named George Riebau. He had to deliver the newspapers early on Sunday morning, and often feared he would be late for chapel. Riebau was a capable bookseller. His shop was visited by intelligent customers, whom Faraday had the advantage of meeting. A French artist named Masquerier lodged with Riebau over the shop. He was a refugee of some distinction, who had painted Napoleon's portrait before he had left France. Faraday had to dust his room and black his boots. Masquerier liked him and gave him lessons in perspective drawing, which proved helpful when he wished to illustrate notes of scientific lectures. It is interesting to notice that both Davy and Faraday had intimate contact in their youth with French refugees.

Faraday learned bookbinding. He became deeply interested in books, and used all his opportunities for reading those of interest which passed through his hands. He first became interested in science by the article on *Electricity* in an encyclopædia that he had to bind. He was particularly delighted with Mrs. Marcet's *Conversations on Chemistry.*

Riebau allowed him "to go occasionally of an evening to hear the lectures delivered by Mr. Tatum on natural philosophy at his house, 53 Dorset Street, Fleet Street." As the lectures were delivered at 8 p.m., Faraday must have had a long working day. The fee for each lecture was one shilling. Faraday's brother Robert, who was three years older and a blacksmith, paid the fees. These Fleet Street lectures were the ancestor of the Birkbeck College. Faraday became acquainted at Tatum's with Benjamin Abbott, a Quaker youth younger than himself, but well-educated. Faraday respected his knowledge and ability. He made other congenial friends, such as Phillips and Nicol. Their companionship stimulated his increasing interest in science.

One of Riebau's customers was Mr. Dance, a member of the Royal Institution. Dance had the kindness and judgment to give Faraday tickets for Davy's last course of lectures. He made beautifully neat notes, with excellent illustrations through the art he had learned from Masquerier. He now began seriously to consider how he might obtain work in connection with science. "The desire to be engaged in scientific occupation, even though of the lowest kind, induced me, whilst an apprentice, to write, in my ignorance of the world and simplicity of my mind, to Sir Joseph Banks, then President of the Royal Society. Naturally enough, 'No Answer,' was the reply left with the porter."

He started to correspond with Abbott, while still an apprentice at Riebau's. In his first letter he excuses himself for starting the correspondence by commenting that "In general, I do not approve of the moral tendency of Lord Chesterfield's letters, but I heartily agree with him respecting the utility of a written correspondence." He explains that he is lacking in a knowledge of grammar and the art of composition and would like to improve his knowledge by corresponding with Abbott on philosophical matters. "I have lately made a few simple galvanic experiments, merely to illustrate to myself the first principles of the science." He had made a small Voltaic battery. "It was sufficient to produce the decomposition of sulphate of

magnesia—an effect which extremely surprised me; for I did not, could not, have any idea that the agent was competent to the purpose." He notes various phenomena, and ascribes them to the impurity of the water-supply, for he has found that the water contains muriatic acid and other substances. He remarks that copper had passed through the flannel divisions of the pile, on to the zinc discs, and zinc had passed through on to the copper discs, which implied that "they must have passed each other. I think this circumstance well worth notice." He describes his inspection of an alleged perpetual motion machine, and found that it had to be wound up once in fourteen days. Its value was estimated at "fifty guineas, or, more definitely, 52 l. 10 s., guineas change in value so much nowadays." In subsequent letters Faraday describes his experimental researches in further detail. He remarks that "Ideas and thoughts often spring up in my mind, and are again irrevocably lost for want of noting at the time." He has noticed that "Several of the metals, when rubbed, emit a peculiar smell, and more particularly tin. Now, smells are generally supposed to be caused by particles of the body that are given off. If so, then it introduces to our notice a very volatile property of those metals. But I suspect their electric states are concerned." Faraday has seen and fully grasped Davy's experiments proving the elementary nature of chlorine. He enthusiastically explains them in detail to Abbott, who, he discovers, supports the old theory and is slightly bored. He is then extremely apologetic, "Pity me, dear A., in that I have not sufficiently the mastery of my feelings and passions. . . . I am fearful that I was influenced by thinking that I had a superior knowledge in this particular subject. Being now aware of this passion, I have made a candid confession of it to you, in hopes to lessen it by mortifying it and humiliating it. You will understand that I shall not now enter on euchlorine until it is convenient for both of us. . . ." In another letter he rallies Abbott, who has complained of his own shortage of philosophical ideas. He writes that a philosopher cannot fail to abound in subjects. As for himself, he is merely short of time. "Oh

that I could purchase at a cheap rate some of our modern gents' spare hours, nay, days; I think it would be a good bargain both for them and me."

Faraday's apprenticeship with Riebau expired in September, 1812, and he was engaged as a journeyman bookbinder by a French *émigré* named De La Roche, a man of passionate disposition, who nevertheless liked Faraday, and offered to make him his heir if he would remain with him. He continued to write to Abbott. In the first letter after he had left Riebau he writes that he "would much rather engage the good opinion of one moral philosopher who acts up to his precepts, than the attention and commonplace friendship of fifty natural philosophers." He is well aware of his "own nature, it is evil," and he "feels its influence strongly." He considers mere "morality only as a lamentably deficient state." His unhappy life under De La Roche stimulated him to attempt again to obtain some sort of scientific work. Under the encouragement of Mr. Dance, who had taken him to Davy's lectures, he wrote to Davy a letter, which, according to S. P. Thompson, is "an astounding example of the high-flown cringing style in vogue at that date." He enclosed his beautiful notes, as an example of his work, and asked for a job.

Unlike his predecessor in the Presidential Chair of the Royal Society, Davy replied, to his immortal credit, with the letter that has already been quoted. He engaged Faraday for some days as an amanuensis, after he had been wounded in the eye during experiments with nitrogen chloride. He tried to dissuade him from becoming a scientist, and recommended him to stick to bookbinding. He promised to send all of the Royal Institution's bookbinding orders to him, and to recommend him as a bookbinder to his friends. Some weeks later, Davy had to sack his assistant, and thought of offering the position to Faraday. So one evening, while Faraday was undressing upstairs, a carriage drew up before the door of his home, and Davy's footman left a note requesting him to call next day at the Royal Institution. Davy asked him whether he still desired to be engaged in scientific work, and then

offered him the vacant position, with two rooms at the top of the Royal Institution, and twenty-five shillings per week.

Faraday was then twenty-one. His letters to Abbott show that his robust intelligence had already obtained a considerable knowledge of science. He had begun to experiment, and his observations had already shown marks of originality. As the letters of a journeyman bookbinder they are remarkable, but not particularly remarkable as the letters of a first-class genius who had already reached manhood. Faraday was not precocious. He was born poor, but taught a creed whose narrowness enabled him to adapt himself to poverty as well as possible. He had a good and intelligent employer in his youth. He had the luck to be engaged by Davy at the Royal Institution. By the age of twenty-one, his account of good and bad fortune had at least balanced, and then it immediately tilted heavily on to the credit side. He became one of the most fortunate of men.

As soon as he had settled in the Royal Institution he began to study the art of lecturing. "It may, perhaps, appear singular and improper that one who is entirely unfit for such an office himself, and who does not even pretend to any of the requisites for it, should take upon him to censure and to commend others." He then explains to Abbott the features that he considers desirable in a lecture room, such as ventilation and the arrangement of entrances and exits. Polite audiences "expect to be entertained not only by the subject of the lecture, but by the manner of the lecturer; they look for respect, for language consonant to their dignity, and ideas on a level with their own. The vulgar—that is to say in general, those who will take the trouble of thinking, and the bees of business—wish for something that they can comprehend. . . . Lastly, listeners expect reason and sense, whilst gazers only require a succession of words." In his next letter to Abbott he continues his theory of lecturing. The lecture should be delivered at a time convenient to the sort of audience for which it is intended. The lecture table should not be cluttered with apparatus, and experiments should be evenly

spaced through the discourse. "The most prominent requisite to a lecturer, though perhaps not really the most important, is a good delivery. . . . A lecturer should appear easy and collected, undaunted and unconcerned, his thoughts about him and his mind clear and free for the contemplation and description of his subject. His action should not be hasty and violent, but slow, easy and natural, consisting principally in changes of the posture of the body, in order to avoid the air of stiffness or sameness that would be otherwise unavoidable. His whole behaviour should evince respect for his audience, and he should in no case forget that he is in their presence. No accident that does not interfere with their convenience should disturb his severity, or cause variation in his behaviour; he should never, if possible, turn his back on them, but should give them full reason to believe that all his powers have been exerted for their pleasure and instruction."

He would allow a lecturer to write out his matter, but not read it. He much disapproves of breaks in the discourse. "One hour is long enough for anyone." "A lecturer falls deeply beneath the dignity of his character when he descends to angle for claps." "I have causelessly heard him condemn his own powers. I have heard him dwell for a length of time on the extreme care and niceness that the experiment he will make requires. I have heard him hope for indulgence when no indulgence was wanted, and I have even heard him declare that the experiment now made cannot fail from its beauty, its correctness, and its application, to gain the approbation of all."

Earlier in this letter Faraday has written, "As when on some secluded branch in forest far and wide sits perched an owl, who, full of self-conceit and self-created wisdom, explains, comments, condemns, ordains and orders things not understood, yet full of his importance still holds forth to stocks and stones around—so sits and scribbles Mike."

Faraday's critique of lecturing was the most remarkable of his early achievements.

Davy had recently been knighted, retired from the Royal Institution and married. He proposed to make a grand

tour of Europe and the Near East, and invited Faraday to accompany him as assistant in experiments and writing. So a few months after he had joined the Royal Institution Faraday had the extraordinary luck of travelling through Europe with the most brilliant British chemist of the day. He had the opportunity of meeting the most distinguished scientists of many countries, and of seeing the world. The observant author of the arts of lecturing was splendidly fitted to benefit from this opportunity. The tour lasted one and a half years.

He had never before been more than twelve miles from London. He was first impressed by the scenery of Devonshire, which he had seen on the way to Plymouth, their port of departure. England and France were at war, but Davy had secured permission for his party to enter France, so they sailed from Plymouth to Morlaix. At the French port they were searched, and detained, until instructions that they should be allowed to proceed were received from Paris. He was shocked by the apparent poverty of the people and countryside. He behaved like a discreet but convinced British patriot. In Paris he went to the Police Department to obtain a passport. "An American, who was there and (perceiving me at a loss for French) had spoken to me, would scarcely believe his senses when he saw them make out the paper for a free Englishman, and would willingly have been mighty inquisitive."

In the next few weeks he met Ampère, Clement, Desormes, Chevreul, and Gay-Lussac. He saw Napoleon visiting the senate in full state. "He was sitting in one corner of his carriage, covered and almost hidden from sight by an enormous robe of ermine, and his face overshadowed by a tremendous plume of feathers that descended from a velvet hat. The distance was too great to distinguish the features well, but he seemed of a dark countenance and somewhat corpulent. His carriage was very rich and fourteen servants stood upon it in various parts."

Davy continually worked on the newly discovered iodine that was brought to his notice by Ampère and others. After three months in Paris the party started for Italy;

Davy continuing to work on iodine as they travelled; and looking for it in the Mediterranean. At Florence they burned diamonds in oxygen with the help of the Grand Duke of Tuscany's great lens. They examined Vesuvius, and feasted on its sides. "Cloths were now laid on the smoking lava, and bread, chickens, turkey, cheese, wine, water and eggs roasted on the mountain, brought forth, and a species of dinner taken in this place. Torches were now lighted, and the whole had a singular appearance; and the surrounding *lazzaroni* assisted not a little in adding to the picturesque effect of the scene. After having eaten and drunk, Old England was toasted, and 'God save the King' and 'Rule, Britannia!' sung; and then two very entertaining Russian songs by a gentleman, a native of that country, the music of which was peculiar and very touching."

The young ex-bookbinder had entered another social world. He travelled on and on through Italy, Switzerland and Germany, increasing his acquaintance among scientists of international reputation. His tour became one of the classical journeys in the history of science. It gave Faraday important direct knowledge of many men of first-class ability, and helped to widen the narrowness left by his youthful training. Contact with first-class minds is one of the chief values of a good education. Many capable young men are unable to learn soon enough the quality of the best minds and hence the relative quality of their own. But the tour was not a perpetual joy for him. Davy had been unable to engage a valet, and gradually added valeting to Faraday's other duties. Lady Davy wished to treat him purely as a personal servant. Faraday resented this attitude, and opposed it. She began to scold him, but with each row she lost ground to the obstinate Sandemanian. Faraday wrote to his friend Abbott that he had seriously considered ending his engagement, but had decided to complete it, as the opportunity of working with Davy was not to be missed. "I should have but little to complain of were I travelling with Sir Humphry Davy alone, or were Lady Davy like him; but her temper makes it oftentimes go wrong with me, with herself and with Sir H. . . ."

He writes that when he returns home he will probably return to bookbinding, "for books continue to please me more than anything else." While Davy was away the financial difficulties of the Royal Institution increased, and its closure was discussed. There was a possibility that it would no longer be in existence when Faraday came back to England.

In February, 1815, the party was in Rome. "Sir H. is now working on the old subject of chlorine, and, as is the practice with him, goes on discovering." He had discovered a new oxide of chlorine.

Davy had intended to visit Turkey and the Near East, but he objected to the Turkish quarantine examination, and the party returned to England rather suddenly. In spite of its insecurity the Royal Institution continued, and Faraday was engaged again as assistant in the laboratory and mineralogical collection, and superintendent of the apparatus, with a salary of thirty shillings a week.

He was soon working intensively, as Davy's assistant, on the miner's safety-lamp. In 1816 he gave his first lecture. This was delivered to the City Philosophical Society, which had been founded by Mr. Tatum, and in the same year his first small research was published. Davy had requested him to analyse some native caustic lime, which had been sent from Tuscany. The little paper contained about four hundred words by Faraday, and additional comments by Davy.

Faraday was now nearly twenty-five years old. He wrote that "It was the beginning of my communications to the public, and its results very important to me. Sir Humphry Davy gave me the analysis to make as a first attempt in chemistry at a time when my fear was greater than my confidence and both far greater than my knowledge; at a time also when I had no thought of ever writing an original paper on science." It is interesting to learn that Faraday had no expectation before the age of twenty-five of ever accomplishing research. His correspondence with Abbott shows a good deal of confidence. Perhaps he was unable to imagine that his private experiments could belong

to the same order of activity as "research." He was overawed by the word. In spite of his very close contact with research and his publication of many papers, Faraday did not publish any important discovery until 1820, when he was twenty-nine. Considering its final magnitude, his genius developed slowly. In 1818 he published some interesting observations on the passage of gases through tubes. He found that at high pressures the mobility of gases decreases as their specific gravity increases, but at low pressures the order of mobility is reversed, as those which traverse quickest when the pressure is high, move more tardily as it is diminished, until they are amongst those which require the longest time in passing the tube. Ethylene escaped from a long fine tube quicker than hydrogen, but when the tube was shortened, the hydrogen escaped twice as quickly. In the same year he demonstrated that musical flames could be made from any combustible gas. The sounds were due to a rapid series of small explosions, as the combustible gas combined with oxygen from the air. The rapidity of repetition of the sounds produced the musical note.

His announcement in 1820 of his discovery of two chlorides of carbon received much attention. The elementarity of chlorine had been suggested to Davy partly by the failure of chlorine and carbon to combine when an electric arc is made in the gas. The existence of compounds of chlorine, hydrogen and carbon, and of phosgene, had suggested to Davy that chlorine and carbon were not without chemical affinity. Faraday found that an excess of chlorine could remove the hydrogen from Davy's compound of chlorine, hydrogen and carbon. The substance left could be crystallized. It fused at a temperature of 320° F. He made a large number of experiments on its reactions with other substances. He found that when it was heated to redness it decomposed, and another chloride of carbon was produced. At room temperature the second chloride was a highly limpid fluid, and boiled at about 160° F.

It has been employed recently in the Pyrene fire-extinguishers. When the liquid is squirted on a small fire

it is immediately converted into a heavy non-inflammable vapour nearly six times as dense as air, and acts as a suffocating blanket. In collaboration with Phillips he showed that some feathery crystals, found in iron retorts in a chemical Factory at Abo, in Finland, and sent to him by M. Julin, were a third variety of chloride of carbon.

At this time Faraday collaborated with Stodart in a detailed attempt to "ascertain whether any alloy could be artificially formed, better, for the purpose of making cutting instruments, than steel in its purest state; and secondly, whether any such alloys would, under similar circumstances, prove less susceptible of oxidation; new metallic combinations for reflecting mirrors were also a collateral object of research."

After working for five years without obtaining any result immediately very useful, this research was dropped, owing to the death of Stodart, and to Faraday's increasing interest in other branches of science. In later years Faraday used to present his friends with razors made from some of his specimens of steel. Sir Robert Hadfield has recently submitted the steel specimens left by Faraday to a thorough investigation. It appears that Faraday and Stodart prepared the first steel that might be considered "stainless." It contained 50 per cent of platinum, and therefore was too expensive to be of much use. The collaborators made the first systematic examination of the effects of additions of varying concentrations of different metals to steel. Faraday developed the structure of his chromium steel by heating its polished surface; this was the invention of the process of "heat-tinting." One of his samples of steel contained only 0·07 per cent. of carbon. Seventy years later, in 1894, when Arnold made his classical researches on the Influence of Elements on Iron, he prepared steels with a lower carbon content in only three cases out of ten, in spite of his vastly superior equipment. If Faraday had retained his interest in steel he might have created a large part of modern metallurgy. He was not inspired to suspect that he was near to several fundamental metallurgical discoveries.

His first great discovery was made in 1821. Ørsted of

Copenhagen had said, in 1807, that he would search for the effect of an electric current on a magnetic needle. He discovered it in 1820. If a magnet is near to a wire carrying an electric current it is deflected. This fundamental phenomenon was not observed until twenty years after the invention by Volta of a battery producing a steady electric current. Scientists had suspected a connection between electricity and magnetism, but apart from the observation that steel was magnetized after being struck by lightning, very little evidence for the connection was discovered. St. Augustine had compared the phenomena of electrical and magnetic attraction, for he had pointed out that rubbed amber would attract any substance if it were light enough, while magnets would attract only iron-containing substances. The failure of the world's acutest scientific minds to detect a connection between magnetism and electricity, though expectant of the possibility, until twenty years after the production of steady electric currents, is an interesting illustration of the difficulty of scientific research. Though Ørsted's account of his experiments contained many acute statements, the notions of his results, often communicated by hearsay rather than reading, were rather vague. He had himself remarked that the magnetic forces appeared to act in circles round the wire. Ørsted's discovery was extended swiftly and brilliantly by Ampère, who found that parallel currents flowing in the same direction attract each other. He proved that the force exerted by a closed circuit on an element of another circuit is at right-angles to the line joining them, by showing that a conductor is not moved along its length by the presence of other conductors. He succeeded in describing these results mathematically by imagining the currents split into short sections, and then deducing the total effect of attractions and repulsions between all of these elements in any electro-magnetic system. This remarkable exhibition of experimental and theoretical power was based on the conception of forces acting along straight lines. Ampère was the first, in 1821, to suggest, that the deflection of a magnetic needle by a current in a wire could be used as an electric telegraph. Besides

Ampère, other scientists also quickly made important contributions. Arago observed that a rotating copper disc could disturb a magnetic needle. Biot and Savart proved experimentally that in the neighbourhood of a straight conductor the force varies inversely as the distance. Arago found that a cylindrical coil of wire acted like a magnet. Arago and Davy observed the magnetization of iron filings by a current in a wire.

Ørsted's first observations suggested that a rotatory movement ought to be obtainable from the interaction of electricity and magnetism. Wollaston deduced that a wire conducting a current ought, in the presence of a magnetic pole, to rotate on its own axis. With Davy's collaboration he tried to make the experiment, but did not succeed.

Faraday repeated for himself the experiments of Ampère, and independently added new experiments proving points for which Ampère had provided theoretical evidence only. During these experiments he convinced himself that "all the usual attractions and repulsions of the magnetic needle by the conjunctive wire are deceptions, the motions being not attractions or repulsions, nor the result of any attractive or repulsive forces, but the result of a force in the wire, which, instead of bringing the pole of the needle nearer to, or further from the wire, endeavours to make it move round it in a never-ending circle and motion whilst the battery remains in action. I have succeeded not only in showing the existence of this motion theoretically, but experimentally, and have been able to make the wire revolve round a magnetic pole, or a magnetic pole round the wire."

By these experiments Faraday became the inventor of the electric motor. The discovery placed him beside Ørsted, Ampère, Arago and the other famous physicists as one of the leading investigators of electro-magnetism, but it involved him in a serious misunderstanding. He had seen something of the attempts of Wollaston and Davy to make a conductor rotate under its own current, and they assumed he had taken Wollaston's idea without acknowledgment. Faraday had the unpleasant task of convincing

them that he had not understood what Wollaston was attempting and that he had developed his experiments and ideas independently. In 1823 Faraday was proposed as a candidate for Fellowship of the Royal Society. Though Wollaston supported his candidature, Davy opposed it vigorously. Davy even requested Faraday to ask his proposers to withdraw his name, but Faraday said that as he had not proposed himself he could not have the proposals withdrawn. Davy then said that, as President, he would remove his name from the list, to which Faraday replied, that he was sure Davy would do what was best for the Royal Society. Davy's jealousy of Faraday was partly due to vanity, and the common failing of a master to appreciate in a pupil talents as great as his own. The misunderstanding over the electro-magnetic rotations was followed by another over the liquefaction of chlorine. In 1810 Davy had shown that the solid substance obtained by exposing chlorine, as usually prepared, to a low temperature, was not liquid chlorine, but a compound of chlorine and water. Taking advantage of cold weather, Faraday decided to analyse some specimens of this hydrate. Davy happened to visit the laboratory while experiments were proceeding and suggested that if the specimens were exposed to heat under pressure, interesting results might be obtained. Faraday followed the suggestion and heated the solid in one leg of a strong bent closed tube. He obtained an oily liquid in the other leg, which proved to be liquid chlorine. In his capacity as President, Davy added a note to the paper that Faraday wrote for the *Philosophical Transactions*, in which he virtually claimed nearly all the credit for the discovery. Faraday's lack of complete originality in this research was used by Davy as an argument against his election to the Royal Society. In spite of Davy's initial opposition, Faraday was elected a Fellow in 1824. Only one black ball was counted against him. Years later Faraday told a friend that his relations with Davy after the election changed, and never became as cordial as before.

In the next year Faraday wrote an account of all the investigators who had liquefied chlorine before both of them!

Particularly Northmore. Davy immediately applied the method to the condensation of hydrochloric acid gas, and Faraday to the condensation of sulphur dioxide, hydrogen sulphide, carbonic acid, euchlorine, nitrous oxide, cyanogen and ammonia. He discovered afterwards that others had already condensed some of these gases. In 1844 Faraday made important extensions to the knowledge of the liquefaction of gases. These will be described later.

In 1825 Faraday read a long paper to the Royal Society on new compounds of carbon and hydrogen. A Mr. Gordon had given him considerable quantities of a liquid obtained during the compression of oil-gas. This gas was prepared by decomposing whale oil at a red heat and was stored at a pressure of 30 atmospheres in portable iron cylinders. It was used for household illumination. Sir Walter Scott used the gas for illuminating his house at Abbotsford and was Chairman of an oil-gas company in Edinburgh. Faraday showed that the liquid was a mixture of substances. One of these was a colourless transparent liquid, with an odour partly resembling that of almonds. Its boiling-point was about 186° F., and its freezing-point about 42° F. "It contracts very much on congealing, 9 parts in bulk becoming 8 very nearly." He showed it was a compound of hydrogen and carbon, and determined the proportions. He named it bicarburet of hydrogen and explored some of its chemical properties. This substance is now known as benzene. In 1845 Hofmann detected it in coal-tar. It has become the chemical raw material of the synthetic dye-stuffs industry.

In 1825 Faraday was appointed a member of a Royal Society committee for the investigation of optical glass. The investigations were made in a laboratory at the Royal Institution, under Faraday's direction. Like those on steel alloys, these researches on glass proved very laborious and not immediately useful. They were completed in four years and became the subject of the Bakerian Lecture for 1829. The memoir was so long that three sittings were required for its hearing. Its most important result proved afterwards to be the preparation of heavy glasses of high

refractive index out of lead boro-silicate. In 1825 Faraday's position at the Royal Institution was improved. Hitherto he had nominally been the assistant of Davy and Brande. On Davy's recommendation he was appointed Director of the Laboratory under the superintendence of Brande, the Professor of Chemistry. His new status enabled him to revive the Institution's organization. Brande was not a brilliant lecturer and the Institution was not flourishing. Faraday started evening meetings, at which members could see experiments and discuss researches in progress at the Institution and elsewhere. In 1826 he formalized these meetings as Friday Evening Discourses. Seventeen were delivered in the first year, of which Faraday himself gave six. These discourses are still continued and have become one of the most famous series of lectures on the progress of science. He extended the appeal of the Institution by starting in 1826–27 the Christmas Courses of Lectures Adapted to a Juvenile Auditory. The first course was given by Wallis on Astronomy, and the second by Faraday on Chemistry. He gave no less than eighteen more courses, the last being in the winter of 1860–61. Faraday himself continued the morning lectures which Davy had in his day made the resort of fashion. Besides expending so much energy on increasing the popularity of the Institution by lecturing, Faraday devoted the most minute care to economy of management. In 1827 he declined the Chair of Chemistry in the newly-founded London University, because "I think it a matter of duty and gratitude on my part to do what I can for the good of the Royal Institution in the present attempt to establish it firmly. The Institution has been a source of knowledge and pleasure to me for the last fourteen years; and though it does not pay me in salary what I *now* strive to do for it, yet I possess the kind feelings and good-will of its authorities and members, and all the privileges it can grant or I require; and, moreover, I remember the protection it has afforded me during the past years of my scientific life. These circumstances, with the thorough conviction that it is a useful and valuable establishment, and the strong hopes that exertions will be followed

with success, have decided me in giving at least two years more to it, in the belief that after that time it will proceed well, into whatever hands it may pass." In spite of the most rigid economy the financial position did not improve. He told the manager of the Institution that "we were living at the parings of our own skin." In 1832 the financial position became acute and a committee of investigation reported that "The Committee are certainly of opinion that no reduction can be made in Mr. Faraday's salary—£100 per annum, house, coals, and candles; and beg to express their regret that the circumstances of the Institution are not such as to justify their proposing such an increase of it as the variety of duties which Mr. Faraday has to perform, and the zeal and ability with which he performs them, appear to merit."

Faraday was then forty-one. In the previous year he had made his most famous discovery. He had already received some of the highest honours bestowed by international learning. Yet his contribution to civilization, his wonderful lecturing, and his economical management were insufficient to attract adequate funds for the conduct of the Institution. Two reflections are suggested by this remarkable circumstance. First, the vulgarity of nineteenth-century culture; and second, Faraday's inability to practise suitable methods of obtaining financial support. The limitations of his personality prevented him from exploiting the social environment of his period. His command of social environment was inferior to Davy's. Faraday avoided corruption by a commercial society, but by retreat and not by conquest. The events of his life have usually been considered very odd and an illustration of his goodness. They can be interpreted equally well as an illustration of the badness of society. Why was the greatest of experimental scientists allowed to receive, after full achievement and fame, "£100 per annum, house, coals, and candles"? He increased his income by a variety of extra work, but that was his official salary. Further, why had he a narrow creed and unsocial habits, which prevented him from being a fully cultured man and prevented him

from being interesting, except on his scientific speciality? Because he could not, in the adverse environment of his day, have accomplished his vast researches without these sacrifices. The Royal Institution and himself were left in financial insecurity when a contributor to the *Quarterly Review* of 1826 wrote: "The prospects which are now opening to England almost exceed the boundaries of thought; and can be measured by no standard found in history. . . . The manufacturing industry of England may be fairly computed as four times greater than that of all the other continents taken collectively, and sixteen such continents as Europe could not manufacture so much cotton as England does."

The manufacturing industry of England was four times greater than that of all the other continents taken collectively; but some thousand pounds for the secure establishment of Faraday's laboratory was not easily to be found. In 1823 a Committee of Manchester weavers wrote: "The average Wages of some hundreds of Weavers, for four months, as proved from the Books of their Employers, is now in the Manchester papers: Their wages for that period was 4s. 10d. per week, to each. This may suggest to you the condition that Weavers' families must be in."

In 1824 a Committee on Agricultural Wages, of which Lord John Russell was chairman, reported that in the south of England agricultural labourers' wages varied from 8s. to 9s. per week, to 3s. for a single man and 4s. 6d. for a married man.

In 1831 the Yorkshire working classes were more interested in agitating for a *ten hours' day* than in the agitation for the Reform Bill. In 1844 Lord Ashley (who became the Earl of Shaftesbury) stated in a speech for the *Ten Hours Factory Bill* that "a manufacturer informed me that he employed females exclusively at his power-looms . . . gives a decided preference to married females, especially those who have families at home dependent on them for support; they are attentive, docile, more so than unmarried females, and are compelled to use their utmost exertions to procure the necessaries of life. Thus are the virtues, the peculiar

virtues of the female character to be perverted to her injury."

In the same year the Factory Inspector, Saunders, reported that "Amongst the female operatives there are some women, who, for many weeks in succession, except for a few days, are employed from 6 a.m. till midnight, with less than 2 hours for meals, so that on 5 days of the week they have only 6 hours left out of the 24, for going to and from their homes and resting in bed."

In 1801 wheat cost 180s. a quarter. In 1934 it costs about 30s. a quarter,—one-sixth the price. Faraday's family received public relief, and he, a child of nine, was given one loaf, which had to last him for a week. Faraday's habits were formed in that class in which married females were often compelled to use their utmost exertions to procure the necessaries of life. He had learnt the sort of religion which members of that class had to adopt, if they were to refrain from violence. Sandemanianism was merely a special form of the Nonconformist religion, which spread so widely among the working classes produced in the Industrial Revolution. Owing to their depth and his natural inflexibility, he took his class-habits of work and thought into his new mode of life. Besides his administrative labour for the Royal Institution, he published several important papers in 1826. One contained a thorough examination of the mutual action of sulphuric acid and naphthalene. He showed that an acid could be obtained from their interaction and prepared a number of its salts. He named it Sulpho-Naphthallic Acid. This was the first sulphonic acid to be recognized. Sulphonic acids have become important in chemical industry because they are easily made and soluble in water. Many important dyestuffs related to naphthalene and benzene are insoluble in water, but may be converted into soluble sulphonic acid compounds by treating them with sulphuric acid. The process is named sulphonation.

In 1826 he published a note on the fluidity of sulphur at common temperatures. He had noticed an extreme example of super-cooling, in which liquid drops of sulphur were

suddenly crystallized when touched, at a temperature 130° below their normal melting-point. He learned afterwards that Bellani had already studied the phenomenon in 1813. In his note of acknowledgment he writes: "I very fully join in the regret which the *Bulletin Universel* expresses, that scientific men do not know more perfectly what has been done or what their companions are doing; but I am afraid the misfortune is inevitable. It is certainly impossible for any person who wishes to devote a portion of his time to chemical experiment, to read all the books and papers that are published in connection with his pursuit; their number is immense, and the labour of winnowing out the few experimental and theoretical truths which in many of them are embarrassed by a very large proportion of uninteresting matter, of imagination, and of error, is such, that most persons who try the experiment are quickly induced to make a selection in their reading, and thus inadvertently, at times, pass by what is really good."

The scientist of 1934 will be amused by Faraday's embarrassment before the magnitude of the scientific literature of 1826. What would he have said of the vast stream of papers now published in thousands? Scientists nowadays have been heard to say that it often takes less time to repeat than to find an account of an experiment that has already been done.

In 1826 Faraday published a paper in which he suggested the existence of a limit to vaporization. He adapted an argument from Wollaston concerning the existence of a limit to the height of the atmosphere. If a piece of metal is lying in a bottle containing a vacuum, it may emit a small quantity of vapour. Then the particles might be pulled back to the metal by gravity more quickly than they are emitted, and no permanent vapour of the metal would be formed in the vacuum. In possible support of this theory he made a number of interesting observations of phenomena now known to be due to secondary atomic attractions. Indeed, he writes that "there is another force, independent of that of gravity, at least of the general gravity of the earth, which appears to me sufficient to

overcome a certain degree of vaporous elasticity, and consequently competent to the condensation of vapour of inferior tension, even though gravity should be suspended; I mean the force of homogeneous attraction." He had begun to examine some of the phenomena of adsorption. Though he did not proceed very far, he had entered a region of physics now associated particularly with Rayleigh, Pockels, Langmuir and others. Other researches of the same year were on the chemical properties of pure rubber, and of Labarraque's Disinfecting Soda liquid, described as *chloride of oxide of sodium*. It was prepared by treating a solution of sodium carbonate with chlorine gas. It had a very pale yellow colour. Faraday's experiments showed "that the whole of the chlorine had not acted upon the carbonate of soda to produce chloride of sodium and chlorate of soda; that much was in a peculiar state of solution or union which enabled it to withstand ebullition, and yet to act freely as a bleaching or disinfecting agent."

During the ten years following 1821, when he had made his great discovery of electro-magnetic rotations, Faraday published very little research on the phenomena of electricity, but he thought much and made many experiments. About 1822 he made notes of ideas for future trial. Some of them are:

"Convert magnetism into electricity."

"Do pith balls diverge by disturbance of electricities in consequence of induction or not?"

"State of electricity in the interior and on the surface of conductors and on the surface of holes through them."

"Light through gold leaf on to zinc or most oxidable metals, these being poles—or on magnetic bars."

"Transparency of metals. Sun's light through gold leaf. Two gold leaves made poles—light passed through one to the other."

The prescience in these queries resembles that in the famous Queries asked by Newton in his *Optics*. Years later Faraday added to his queries the comment "I already owe much to these notes and think such a collection worth making by every scientific man." When a query

was answered he drew a line through it and wrote the date of solution by the side.

By 1830 Faraday's interests in research had become wide and deep. His reputation as an expert consultant had also grown. In 1830 his income from consulting was about £1,000. The research and the consulting absorbed the whole of his energy. If the quantity of either had to be increased, it had to be at the expense of the other. Was he to turn more to research or to consulting? He decided in 1831 that he would turn entirely to research. His income in that year from consulting fell to £155 and in every year afterwards was still less. In the society of the nineteenth century a famous man of thirty-nine years could have little convenience without some degree of wealth. At the most critical age and point in his career, Faraday suddenly reduced his income by two-thirds because he wished to devote the whole of his energy to research. A few months after he had made the decision he found the answer to his query of how to "convert magnetism into electricity." This is one of the most fundamental discoveries. It is to be classed with the discovery of current electricity by Volta, of electro-magnetism by Ørsted, of X-rays by Röntgen, and radio-activity by Becquerel. It was not quite so original as these because many acute physicists believed that magnetism must be convertible into electricity, but they could not find out how. But Faraday's success in competition with the world's best physicists showed his mastery of experimental technique. Further, he explored the meaning of his discovery in an almost unique manner. For twenty-five years after 1831 he pursued every aspect of electro-magnetic phenomena until he had accumulated a complete introductory knowledge of electro-magnetism. During these years he gradually created a complete descriptive theory of electro-magnetism. Faraday's discovery of electro-magnetic induction, followed by twenty-five years' detailed research into its implications, and the successful formulation of a descriptive theory of the experimental observations, is one of the greatest scientific achievements. It places him beside Newton, Lavoisier, Darwin and Mendel, and above Volta,

Ørsted, Röntgen, Becquerel and other discoverers of fundamental but isolated facts.

The discovery of electro-magnetic induction was a critical point in the progress of science, and followed a critical decision in Faraday's life. In 1831 his scientific vision was extended to vast regions, and his dedication left him free to devote the rest of his life to a survey of those unknown regions of natural phenomena. After 1831 he began gradually to contract his social life. Faraday had lived as livelily as his creed permitted. He was not ascetic. He robustly enjoyed all pleasures not denied to him by principle. The Sandemanian youth had thoroughly enjoyed feasts on the sides of Vesuvius and Italian carnival masques. A dozen years later he was still in the habit of romping after dinner in his rooms at the Royal Institution. Sometimes he used to ride with his young cousins round the Institution's almost sacred theatre on a velocipede. His lasting interest in books and printing made him friendly with the lithographer Hullmandel, who held very pleasant conversaziones of artists, actors and musicians. Hullmandel used to take his parties up the Thames in an eight-oared cutter, in which they cooked their own dinner, and were entertained by the singing of Garcia and his daughter, Malibran, and other friends. Turner joined these parties, and came to consult him about the chemistry of pigments.

During his country and seaside holidays he was an indefatigable naturalist. He enjoyed reading aloud, especially Byron's "Childe Harold" and Coleridge's "Mont Blanc." He could not bear indecision. He believed that in little things quickness of decision was important and a bad decision better than none. He did not read widely, but when he was tired he sometimes read exciting novels or went to the theatre. He was fond of classical music and in his youth could sing many songs and played the flute.

This evidence shows that Faraday was deeply extroverted. In his social life he was an amiable but ordinary man. Outside his science he was devoid of subtle intellectual feeling. He understood few things, but

accomplished prodigious works. He was the opposite of his acquaintance Coleridge, who understood so many things and accomplished so little.

Before 1831 he had published sixty papers on research. His robust power of experiment and observation had ranged over a variety of subjects. Afterwards it was concentrated chiefly on to the phenomena of electricity.

Faraday published in July 1825 a short note of an unsuccessful experiment. The complete note may be quoted: "As the current of electricity, produced by a Voltaic battery when passing through a metallic conductor, powerfully affects a magnet, tending to make its poles pass round the wire, and in this way moving considerable masses of matter, it was supposed that a reaction would be exerted upon the electric current capable of producing some visible effect; and the expectation being, for various reasons, that the approximation of a pole of a powerful magnet would diminish the current of electricity, the following experiment was made. The poles of a battery of from two to thirty 4-inch plates were connected by a metallic wire formed in one part into a helix with numerous convolutions, whilst into the circuit, at another part, was introduced a delicate galvanometer. The magnet was then put in various positions and to different extents, into the helix, and the needle of the galvanometer noticed; no effect, however, upon it could be observed. The circuit was made very long, short, of wires of different metals and different diameters down to extreme fineness, but the results were always the same. Magnets more or less powerful were used, some so strong as to bend the wire in its endeavours to pass round it. Hence it appears that however powerful the action of an electric current may be upon a magnet the latter has no tendency, by reaction, to diminish or increase the intensity of the former, a fact which, though of a negative kind, appears to me to be of some importance."

This remarkable note might have had diverse effects on readers of diverse temperaments. Some might have been frightened away from repetition of the experiment by the clarity of its negative results, and others might have followed

the hint of passing magnets through wire coils and discovered the desired effect. This note may have diverted possible competitors of Faraday from the search. It might also have given them the hint of the solution. But it is certain that Faraday was not frightened by his own note, and was able to take his own hint. It is said that he used to carry a little bar of iron and a small coil of wire in his waistcoat pocket, and would bring these out at spare moments and try to imagine by what sort of arrangement the iron bar could produce an electric current in the coil.

In Faraday's diary there is a note on November 28th, 1825, of an attempt to discover the production of an electric current in one circuit by an electric current in another circuit. He connected the terminals of a battery by a wire about four feet long. Another wire was arranged parallel to the first wire and separated only by two thicknesses of paper. The ends of the second wire were connected to a galvanometer. He was unable to observe that any current flowed in the second wire while a current from the battery was flowing through the first wire. He tried the effects of connecting the battery terminals with a wire wound into a coil, and passing the second wire through the hole of the coil. Then he tried the effect of connecting the battery terminals by a straight wire which passed through the hole of a wire coil whose ends were connected to a galvanometer. These experiments also showed no production of electric current in the galvanometer circuit. In 1828 he tried similar experiments, without success. Three years later he discovered an effective arrangement. On August 29th, 1831, he described in his diary an experiment with "an iron ring made, iron round and seven-eighths of an inch thick and six inches in external diameter. Wound many coils of copper round, one-half of the coils being separated by twine and calico: there were three lengths of wire, each about twenty-four feet long, and they could be connected as one length or used as separate lengths. By trial with a trough each was insulated from the other. Will call this side of the ring A. On the other side, but separated by an interval, was wound wire in two pieces, together

amounting to about sixty feet in length, the direction being as with the former coils. This side call B. Charged a battery of ten pairs of plates four inches square. Made the coil on B side one coil, and connected its extremities by a copper wire passing to a distance, and just over a magnetic needle (three feet from the iron ring), then connected the ends of one of the pieces on A side with battery: immediately a sensible effect on needle. It oscillated and settled at last in original position. On breaking connection of A side with battery again a disturbance of the needle."

The effect was transient; the current in the second coil was produced only when the current in the first coil was started or stopped. Why had the previous experiments failed, and why was the present observed effect transient? Faraday had rightly expected that if an electric current could deflect a magnetic needle, magnetism should produce a current. But, together with his contemporaries searching for this phenomenon, he had not correctly conceived the nature of Ørsted's experiment. He and they had assumed that the electric current which deflected a magnetic needle was a static phenomenon because it registered a steady deflection on a galvanometer. This was a mistake. The electric current was moving electricity, a dynamic phenomenon. The deflection of the magnetic needle is produced by a dynamic phenomenon. Now a stationary magnet is a static phenomenon. Hence a stationary magnet, or stationary magnetism, cannot produce the converse of a steady current because it is not the same sort of phenomenon. If moving electricity produces magnetism, then moving magnetism will be necessary to produce electricity. In the iron ring experiment the current in coil A produces magnetism in the ring. While this magnetism is being produced, and consequently is in motion, it is able to produce a current in coil B. When the magnetism is raised to a steady state it becomes stationary and the current in coil B disappears. The element of movement in the electric current in Ørsted's experiment had been overlooked, or not comprehended, hence investigators had not looked for effects with moving magnets. In 1825 Faraday had placed

a magnet in a coil and then looked for an effect; but he had not looked for an effect while he was placing the magnet in the coil. The transformation of electricity into magnetism and vice versa depends on relative motion. As Sir William Bragg has explained, this is the point from which the theory of relativity begins to take shape. Faraday's greatest achievement was not the discovery of how to obtain electricity from magnetism, but his elaboration of a descriptive theory of how it occurred. The theory of relativity was drawn by Einstein out of Maxwell's mathematical transcription of Faraday's theory of electrical action. Faraday is, therefore, the chief ancestor of the theory of relativity. The problems of relative motion propounded by Newton's investigation of the motions of gravitating bodies were not the starting-point of the theory of relativity. The motion of electricity presented the problems of relative motion in the form in which they inspired the formulation of the theory of relativity. A consideration of the dynamics of the electro-magnetic field led Einstein to study the Newtonian laws of motion. He found Mach's critique of Newtonian mechanics suggestive. But he became interested in the conceptions of relativity through the study of the motions of electricity and not through the study of the motions of gravitating particles.

On August 29th Faraday performed several variations of his chief experiment. On the next day he wrote: "May not these transient effects be connected with causes of difference between power of metals at rest and in motion in Arago's experiments?" On September 24th he tried to induce a current in one coil by making or breaking the current in another coil, but without success. Then he tried the effects of a bar of iron in place of the ring of iron. He wound a coil around a bar of iron. The ends of the coil were connected with another coil containing a galvanometer. The respective north and south poles of two similar bar magnets were placed in contact and their remaining south and north poles joined by the iron bar carrying the coil. Whenever the magnetic contact between either of the magnets and the iron bar were broken a deflection in the

galvanometer was observed. "Hence here distinct conversion of magnetism into electricity."

On October 1st he succeeded in inducing a current in one coil by a current in another coil without the assistance of any piece of iron. In his paper of November he describes his experiment: "Two hundred and three feet of copper wire in one length were coiled round a large block of wood; other two hundred and three feet of similar wire were interposed as a spiral between the turns of the first coil and metallic contact everywhere prevented by twine. One of these helices was connected with a galvanometer and the other with a battery of one hundred pairs of plates four inches square, with double coppers, and well charged. When the contact was made there was a sudden and very slight effect at the galvanometer, and there was also a similar slight effect when the contact with the battery was broken."

On October 17th he discovered that electricity could be produced by moving a magnet near a coil. "A cylindrical bar magnet three-quarters of an inch in diameter and eight inches and a half in length had one end just inserted into the end of the helix cylinder (two hundred and twenty feet long); then it was quickly thrust in the whole length and the *galvanometer* needle moved; then pulled out, and again the needle moved, but in the opposite direction. This effect was repeated every time the magnet was put in or out, and therefore a wave of electricity was so produced from *mere approximation of a magnet* and not from its formation *in situ*."

On October 28th he showed that a current could be obtained by moving a conductor near a stationary magnet. He "made a copper disc turn round between the poles of the great horse-shoe magnet of the Royal Society. The axis and edge of the disc were connected with a galvanometer. The needle moved as the disc moved." On November 4th he found that a copper wire moved between magnetic poles produced the effect. "When a mere wire connected with the galvanometer so as to form a complete circuit was passed through between the poles the galvanometer was affected; and upon moving the wire to and fro so as to make the alternate impulses produced correspond

with the vibrations of the needle the latter could be increased to 20° or 30° on each side the magnetic meridian." He then obtained the same effects when the wire was moved to and fro between the poles of an electro-magnet. In the "moving to and fro" he had invented the first crude dynamo, for he had produced a continued current of electricity by moving a conductor through a magnetic field. Then he proceeds, in his first paper, to state "*the law* which governs the evolution of electricity by magneto-electric induction." He expresses it in terms of the way in which a moving wire cuts the magnetic curves, or moving magnetic curves are cut by a stationary wire. "By magnetic curves I mean the lines of magnetic forces, however modified by the juxtaposition of poles, which would be depicted by iron filings; or those to which a very small magnetic needle would form a tangent." Thus from November, 1831, Faraday defined the laws of electro-magnetism in terms of the *relative motion* between lines of magnetic force and conductors. The notions of the lines of force, of the structure of the electromagnetic field and of the fundamental rôle of relativity are inherent in the first paper on his discovery of electromagnetic induction. During the next twenty-five years he elaborated his vision of the lines of force until in his imagination they became as real as matter. He imagined a model of the universe in terms of these lines, as a model of the universe has been imagined in terms of atoms.

The lines of force, as conceived by Faraday, became the counterpart of the particle as the unit of material nature.

Faraday's conception of the operation of electricity and magnetism through lines of force led him to suggest that space traversed by such lines was in a peculiar condition, named by him the *electro-tonic* state. The difference between space in the ordinary state and in the electro-tonic state was shown by the absence of a current in a wire ring when revolved in space in the ordinary state, and its presence in the ring when revolved in space in the electro-tonic state. Shortly afterwards Faraday rejected the idea as it was not necessary for the explanation of the particular phenomena in connection with which it had been invoked, but many

years later he returned to the belief that the idea was of fundamental importance.

In his first paper on electro-magnetic induction Faraday described the first crude dynamo machine. It consisted of a copper disc which could be rotated between the poles of a pair of magnets. The axle of the disc was connected with one terminal of a galvanometer and a wire from the other terminal was held against the outside edge of the rotating disc. The galvanometer showed a constant deflection when the disc was rotated at a constant speed. "Here therefore was demonstrated the production of a permanent current of electricity by ordinary magnets." Faraday immediately devised a variety of primitive dynamos, but he did not try to develop any of them. He wrote: "I have rather, however, been desirous of discovering new facts and relations dependent on magneto-electric induction than of exalting the force of those already obtained, being assured that the latter would find their full development hereafter."

In his Bakerian Lecture of January, 1832, Faraday considered the effects of electro-magnetic induction in the physics of the earth as Davy had considered the effects of electro-chemistry on the composition of the earth. He showed that the rotation of a coil in the earth's magnetic field produced a current. He explained that the flowing water of a river, and the tide through the Straits of Dover, should produce currents because there was a relative motion between them and the lines of the earth's magnetic field. He made experiments at Waterloo Bridge, but without success. Years afterwards, when the submarine cables were laid across the English Channel and the Atlantic Ocean, electric currents induced in them by water currents were observed.

Besides his sublime researches on electro-magnetic induction, Faraday published two other considerable papers in 1831. One was on the optical deception by which a succession of moving objects produces the appearance of a stationary object. "Being at the magnificent lead mills of Messrs. Maltby, two cog-wheels were shown me moving with such velocity that if the eye were retained immovable

no distinct appearance of the cogs in either could be observed; but, standing in such a position that one wheel appeared behind the other, there was immediately the distinct though shadowy resemblance of cogs moving slowly in one direction." The analysis of this phenomenon exposed the principle of the cinematograph. Faraday learned that Plateau had published, unknown to him, an account of the phenomenon three years earlier. The other paper described an investigation of the difference between the figures made by coarse and by fine sand on vibrating plates. He ascribed the difference to the little vortices of air formed over the places where the motions were of greatest amplitude.

In 1833 Faraday published papers on the identity of the various sorts of electricity. He showed thoroughly that the electricities obtained from Voltaic batteries, from frictional machines, from electro-magnetism and a variety of other sources had identical properties. This research was a forerunner of the classical researches of Mayer and Joule on the conservation of energy. Faraday had a profound belief in the existence of a general principle comprehending all natural phenomena. He consciously made experiments according to this belief. The conservation of energy was for him naturally axiomatic, and his experimental demonstration of the identity of the electricities was partly an exhibition of experimental virtuosity. This work was followed in the same year by the development of the modern conceptions of electro-chemical phenomena. Grothuss, Davy, Berzelius and others had proposed theories of the decomposition of chemical solutions by an electric current. Though these theories founded the modern conceptions of the phenomena they were descriptive and inaccurate. But they should not be undervalued. Admirers of the brilliant correction and quantitative proof of the laws of electrolysis by Faraday have tended to forget that he was not the founder of the subject.

Faraday's first important investigations of electrochemistry are published at the end of his paper on the identity of the electricities. He has compared the quantity of electricity produced by an electro-magnetic machine

with the quantity engaged in a chemical action by noting the size of a steady deflection of a galvanometer needle and counting the length of wire by the beats of his watch. "Hence it results that both in *magnetic deflection* and in *chemical force* the current of electricity of the standard Voltaic battery for eight beats of the watch was equal to that of the machine evolved by thirty revolutions. . . . It also follows that for this case of electro-chemical decomposition, and it is probable for all cases, that the *chemical power, like the magnetic force, is in direct proportion to the absolute quantity of electricity* which passes." Then he published his observation that ice, unlike water, is not a conductor. "As it did not seem likely that *this law of the assumption of conducting power during liquefaction, and loss of it during congelation*, would be peculiar to water I immediately proceeded to ascertain its influence in other cases, and found it very general." This showed that the widely held belief that water was necessary to conduction in non-metallic liquids was untrue. Then he made an experiment in which the products of decomposition of an electrolyte were collected at places separated from the electrodes. This showed that decomposition was not due to forces emanating directly from the electrodes. After further experiments and discussion he writes: "*Judging from facts only*, there is not as yet the slightest reason for considering the influence which is present in what we call the electric current—whether in metals or fused bodies or humid conductors, or even in air, flame and rarefied elastic media—as a compound or complicated influence. It has never been resolved into simpler or elementary influences and may perhaps best be conceived of as an *axis of power having contrary forces*, exactly *equal in amount, in contrary directions*. . . . Passing to the consideration of electro-chemical decomposition, it appears to me that the effect is produced by an *internal corpuscular action*, exerted according to the direction of the electric current, and that it is due to a force either *superadded to* or *giving direction to the ordinary chemical affinity* of the bodies present."

Having convinced himself that a definite quantity of

electricity is always associated with the same quantity of chemical action, he invented the voltameter as an instrument for determining quantities of electricity by measuring the quantity of chemical decomposition they could produce. He found that the instruments sometimes gave anomalous results, owing to the absorption of gases by the metal electrodes, and "the power of Metals and other Solids to induce the Combination of Gaseous Bodies." So he turned aside in order to examine the phenomena of catalysis. At the beginning of his next paper, published in January, 1834, he explains the new terminology he has devised, with the help of friends, for the description of the phenomena of electro-chemistry. The words *electrolyte*, *electrolyze*, *electrode*, *cathode*, *anode*, *cation*, *anion* and *ion* are used for the first time. When Faraday wanted a new term he usually sent a careful description of his requirements to Whewell. The famous terminology of electrolysis was chiefly due to Whewell's excellent etymological taste. Faraday wanted an alternative for "pole" which contained no implication of influence. "Electrode" meant merely the "way for the current." The "ions" were the travelling substances which appeared at the electrodes during the decomposition. Faraday did not apply the name to each individual travelling particle. It was a collective name. He connected a number of electrolytic baths in series so the same current could be sent through all of them. He placed a different electrolyte in each bath, and compared the relative amounts of the substances liberated by the same current in the different electrolytes. He found that the masses of the substances liberated in the various baths are in the ratio of their chemical equivalents. He had already shown that the mass of any substance liberated was proportional to the quantity of electricity which had passed through the electrolyte.

These two laws and his conception of ionization (which was originally a collective, not an individual term) led him to a number of sublime speculations. "It is *probable* that all our present elementary bodies are *ions*." "The simple substances . . . will probably form the first group, and the acids and bases . . . the second group." "I think I

cannot deceive myself in considering the doctrine of definite electro-chemical action as of the utmost importance. It touches by its facts more directly and closely than any former fact, or set of facts, have done upon the beautiful idea that ordinary chemical affinity is a mere consequence of the electrical attractions of the particles of different kinds of matter."

Then he writes a paragraph "On the absolute quantity of Electricity associated with the particles or atoms of Matter." He begins: "The theory of definite electrolytical or electro-chemical action appears to me to touch immedi ately upon the *absolute quantity* of electricity or electric power belonging to different bodies. It is impossible, perhaps, to speak on this point without committing oneself beyond what present facts will sustain; and yet it is equally impossible, and perhaps would be impolitic, not to reason upon the subject. Although we know nothing of what an atom is, yet we cannot resist forming some idea of a small particle which represents it to the mind; and though we are in equal, if not greater, ignorance of electricity, so as to be unable to say whether it is a particular matter or matters, or mere motion of ordinary matter, or some third kind of power or agent, yet there is an immensity of facts which justify us in believing that the atoms of matter are in some way endowed or associated with electrical powers, to which they owe their most striking qualities, and amongst them their mutual chemical affinity. As soon as we perceive, through the teaching of Dalton, that chemical powers are, however varied the circumstances in which they are exerted, definite for each body, we learn to estimate the relative degree of force which resides in such bodies; and when upon that knowledge comes the fact that the electricity which we appear to be capable of loosening from its habitation for a while and conveying from place to place, *whilst it retains its chemical force*, can be measured out, and being so measured is found to be as *definite in its action* as any of *those portions* which, remaining associated with the particles of matter, give them their *chemical relation*, we seem to have found the link which connects the proportion of that we have evolved

to the proportion of that belonging to the particles in their natural state."

Then he describes experiments which prove that the amount of electricity required to decompose a grain of water is extraordinarily large. For instance, 800,000 discharges of a certain Leyden jar would appear to be necessary. Another experiment confirms the "high electric condition of the particles of matter and the *identity as to quantity of that belonging to them with that necessary for their separation.*" . . . "*The electricity which decomposes and that which is evolved by the decomposition of a certain quantity of matter are alike.*" . . . "The harmony which this theory of the definite evolution and the equivalent definite action of electricity introduces into the associated theories of definite proportions and electro-chemical affinity is very great. According to it the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity or have naturally equal electric powers, it being the *electricity* which *determines* the equivalent number, *because* it determines the combining force. Or, if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action have equal quantities of electricity naturally associated with them. But I must confess I am jealous of the term *atom*, for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration." (January, 1834.) As Helmholtz explained in 1881, this implies the existence of the electron or atom of electricity. Faraday did not completely assert the existence of an electron because, following his teacher Davy, he was not sympathetic to atomic theories. He was more sensitive to the limitations than the powers of the atomic theory of chemistry. This theory was not logically established until 1858, when Cannizzaro showed that Avogadro's hypothesis of the distinction between atoms and molecules would explain all apparent contradictions concerning the chemical equivalent weights of the elements. Faraday's chief contribution to science was the conception of the field of physical forces

and his aversion to particulate theories helped him to arrive at his field conceptions. He preferred to think of activity as something which occurred throughout a volume, and was not to be conceived in terms of reactions between particles. He removed the Newtonian particle from its dominating place in physical science.* He could not have been expected to introduce the particle into the theory of electricity while engaged in taking particulate conceptions out of that theory. His recognition in 1834 that his facts forced the suggestion that electricity possessed particulate qualities was the more marvellous because the bent of his mind was against particulate conceptions. In spite of himself, he nearly discovered the electron fifty years before the evidence for its existence became strong. There was also another psychological difficulty in proceeding to the assertion of the existence of an electron from the facts of electro-chemical equivalence. It is not necessary to suppose that the association of a certain quantity of electricity with a chemical atom implies that this quantity of electricity is an indivisible unit. The unit might merely represent the size of the appetite of a chemical atom for an infinitely divisible electric fluid. Human beings are accustomed to drink beer in pints, but this does not prove that a pint of beer is indivisible. The assertion of the existence of an indivisible quantum of electricity from Faraday's electro-chemical facts requires an additional psychological step. There is no evidence that he was prepared to take this step; to assume that the unit of electricity associated with chemical atoms was indivisible and was not merely a certain measure of a continuous fluid. Thus Faraday was probably farther from the discovery of the electron than is suggested by the extraordinary passage that has been quoted. The failure for fifty years of others to make the correct deduction was partly due to Faraday's intellectual ascendency. He had impressed his own perspective of the facts on his successors, and they also were unable to see their full meaning because they considered

* Prof. Vasiliev has informed me that Faraday was influenced in his youth by Euler's theory of fields of force, who in turn had been influenced by Descartes.

them with Faraday's psychology. Faraday's collective use of the term "ion" as the substance which travels through an electrolyte is additional evidence of his distaste for particulate conceptions. He was less fitted to discover the particulate than the field aspects of phenomena.

He followed his investigation of the phenomena of electro-chemical decomposition by detailed investigation of the mode of operation of the Voltaic battery. In this paper he confirms Davy's early belief that the electricity is a product of chemical action. He concludes that the current in a zinc, platinum and dilute sulphuric acid cell is derived from the "mutual affinity of the metal zinc and the oxygen of the water." When the zinc is alone it is not strong enough to take the oxygen from the water, but it is able by its attraction of the oxygen to create "a peculiar state of tension" between the particles of oxygen and the particles of hydrogen in the water. "The state of tension is best relieved by dipping a metal which has less attraction for oxygen than the zinc into the dilute acid and making it also touch the zinc. The force of chemical affinity, which has been influenced or polarized in the particles of the water by the dominant attraction of the zinc for the oxygen, is then transferred, in a most extraordinary manner, through the two metals so as to re-enter upon the circuit in the electrolytic conductor which, unlike the metals in that respect, cannot convey or transfer it without suffering decomposition; or rather, probably, it is exactly balanced and neutralized by the force which at the same moment completes the combination of the zinc with the oxygen of the water. The forces, in fact, of the two particles which are acting towards each other, and which are therefore in opposite directions, are the origin of the two opposite forces, or directions of force, in the current. Being transferred forward in contrary directions, they produce what is called the voltaic current: and it seems to me impossible to resist the idea that it must be preceded by a *state of tension* in the fluid, and between the fluid and the zinc; the *first consequence* of the affinity of the zinc for the oxygen of the water."

Then in the next paragraph, with magnificent intellectual

grandeur, he describes his attempt to discover this state of tension, "conceiving that it might produce something like structure" in the liquid of a Voltaic cell, either before or during discharge. He passes a beam of light through the electrolyte "directly across the course of the electric current" in order to see whether it is polarized. He is unable to observe any effect. He repeats the experiment with borate of lead, which is a glass when solid and an electrolyte when fused; but again without any effect. The magnitude of genius is often better shown by failure than success. In the conception of this experiment Faraday's imagination is seen piercing generations beyond his own time. On the foundation of the facts he had collected experimentally he constructed an imaginative structure that towered into another period of scientific thought.

In the autumn of 1834 Faraday was again investigating induction. Mr. W. Jenkin had informed him that "if an ordinary wire of short length be used as the medium of communication between the two plates of an electrometer consisting of a single pair of metals, no management will enable the experimenter to obtain an electric shock from this wire; but if the wire which surrounds an electro-magnet is used a shock is felt each time the contact with the electrometer is broken, provided the ends of the wire be grasped one in each hand." Faraday said that this was the sole valuable observation among the hundreds that were reported to him by amateurs during his life. From it he elucidated the phenomena of self-induction, showing that the spark obtained by breaking a circuit containing a long straight wire is much less bright than that obtained when the same wire is coiled. He showed also that if the wire is bent double so that the two halves lay parallel together the coil made from this doubled wire is without self-induction, a discovery of great importance for the design of electrical machinery.

He showed that the splitting of a conductor into a number of parallel strands reduces its self-induction. This phenomenon explains the superiority of a copper ribbon over a copper wire for lightning conductors.

At the end of 1835 he began to give special consideration to the nature of common or statical electricity and its relation to current electricity. He resolved to investigate whether statical electricity resides on the surface of a conductor or on the surface of the insulator in contact with the conductor. He noticed that the phenomena of induction indicated that the electricity was in the insulator and not in the conductor. Then he began to suspect that the lines of force from a static electric charge may be curved and capable of going round corners. This idea led to the emancipation of electrical theory from the Newtonian gravitational theory, as the latter depended on the idea of forces acting along the straight line joining two particles. In the theories of Coulomb and Ampère the behaviour of those electric and magnetic forces whose resultants act along straight lines had been discussed. The straight-line, action-at-a-distance conceptions of gravitational theory can be applied only to simple examples of electro-magnetic phenomena. The creation of new and general conceptions suitable to the description of all electro-magnetic phenomena is Faraday's greatest achievement. He showed experimentally that an insulated sphere could be charged by induction even when screened from the direct action of the charge. He found, too, that the induced charge could be increased by disposing the sphere, screen and charge in certain positions which left the sphere and the charge farther apart than before. This increase of charge with increase of distance could not be paralleled from gravitational phenomena. Then he discovered another property of electric forces that could not be paralleled from the properties of gravitational forces. He arranged an insulated metal sphere within another hollow metal sphere which was not insulated, leaving an annular space of about half an inch between the two spheres. He made two exactly similar instruments of this design. Then he charged the inner sphere of one of them and connected it with the inner sphere of the other. He found that the two instruments divided the charge equally when their annular spaces were filled with air, but when one of the spaces was filled with shellac, sulphur or

spermaceti the charge was not equally divided. The instrument with these solid materials took more than half the charge. Hence the shellac, or dielectric, had absorbed some electricity. He found that the dielectric took time to absorb this electricity. When the inner sphere was discharged the electricity slowly came out of the charged dielectric and began to charge the sphere again. Thus he had proved that the medium between conductors, unlike the medium between gravitating bodies, can affect the forces acting between the conductors. Faraday's discovery of "specific inductive capacity" is an interesting example of his power, because the phenomenon had previously been discovered by Cavendish but not published. Faraday repeated the discovery in order to establish his conception of electric action. This achievement shows a certain inevitability in his researches and how he obtained results independent of luck. His detailed investigation of static electrical induction shaped in his mind certain conceptions of its nature. "*Induction* appears to be essentially an action of contiguous particles, through the intermediation of which the electric force, originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind exactly equal in amount but opposite in its direction and tendencies." Then he writes that "The direct inductive force, which may be conceived to be exerted in lines between the two limiting and charged conducting surfaces, is accompanied by a lateral or transverse force, equivalent to a dilation or repulsion of these representative lines," a conclusion he had drawn from an experiment in which he had found that "the induction fairly turned a corner." He imagined the curved lines of the electric field by analogy with the curved lines of the magnetic field. The electrical researches published by Faraday in 1838 occupy 143 pages of his *Experimental Researches*. Besides the innumerable experiments on the nature of electric forces, there is a detailed investigation of brush discharges, and the discovery of the dark discharge from the cathode in air at low pressure is described.

After these stupendous labours Faraday's health broke

MICHAEL FARADAY AND HIS WIFE SARAH

(Mr. W. E. Gray)

[*face* 1

down for a time. For many years he had occasionally suffered from loss of memory and dizziness. When afflicted by these attacks, or feeling mentally tired, he used to go to Brighton, though he thought it not a very attractive place. In 1840 these conditions became more extensive and prevented him from working. He became uncomfortable in the presence of strangers. He found letter-writing a severe strain. He felt that doctors did not understand his illness. He wrote notes which show a slight mental derangement and resemble similar notes written by Newton during a similar period of mental exhaustion. Faraday's mental health broke down in 1840, when he was forty-nine. Newton's mental health broke down in 1692, when he also was forty-nine. In one of his strange notes Faraday wrote: "Whereas according to the declaration of that true man of the world Talleyrand, the use of language is to conceal the thoughts; this is to declare in the present instance, when I say I am not able to bear much talking, it means really, and without any mistake, or equivocation, or oblique meaning, or implication, or subterfuge, or omission, that I am not able, being at present rather weak in the head and able to work no more."

The peculiar reference to "that true man of the world Talleyrand" suggests that he was suffering from mental repression besides exhaustion. When in a similar state Newton accused Locke of trying to "embroil him with women."

There are hints in these notes of how much the normal human feelings have been sacrificed by these scientific geniuses. Faraday and Newton were childless.

After 1831 Faraday had gradually contracted his social activities. He made a note of these in a table. In 1834 he stopped professional business in courts and declined all dining-out or invitations. In 1835 he stopped consulting for the excise business and declined the Council business of the Royal Society. In 1837 he stopped much morning lecturing. In 1838 he declined reprinting *Chemical Manipulations* and "closed three days in the week. (Saw no one)." In 1839 he stopped giving Mr. Brande's

morning lectures. In 1840 he stopped giving juvenile lectures. In 1841 he stopped giving Friday evening lectures, Easter lectures, and "all other business at R.I."

He went into the country to rest, and visited Switzerland. There he often walked thirty miles, and sometimes forty-five miles, in a day. He kept a full diary of his travels, but found letter-writing painful. In 1842 he explained to a scientist, who wished to see him, that he could not see people or even visit the houses of his friends because of "*ill-health connected with my head.*" In 1843 he wrote to Matteuci that one reason why he did not visit the meetings or the Council of the Royal Society was ill-health, and another that he did not like the Society's constitution, and wanted to restrict it to scientific men. His scientific work was entirely suspended for about twenty months. Afterwards he did a little research, including a demonstration that the electrification of steam, observed by Armstrong, was due to friction. By 1844 he had begun to work more regularly. The notes of one of his Easter lectures contains the following: "Final brooding impression, that particles are only centres of force; that the force or forces constitute the matter; that therefore there is no space between the particles distinct from the matter; that they touch each other just as much in gases as in liquids or solids; and that they are materially penetrable, probably even to their very centres. That, for instance, water is not two particles of oxygen side by side, but two spheres of power mutually penetrated and the centres even coinciding."

These conceptions have become the working conceptions for the physicist of 1934. The particle is now conceived as a concentration in a field of waves extending throughout space. The interpenetrability of neutrons and electrons and other particles was established two years ago, after suggestions by Niels Bohr. The dependence of chemical affinity on interpenetrating spheres of power is seen in the new wave-mechanical theories of valency.

Faraday's severe mental illness did not impair the quality of his mind. When he recovered, his mind was as acute as ever, and had apparently generated new ideas during the

period of quiescence. When Newton recovered his equanimity he did not recover his intellectual initiative. His mind also remained acute, but would not voluntarily engage itself on problems. Newton could be persuaded to attack problems, but he would not attack them without being persuaded. Faraday escaped this destruction of initiative. In 1844 he resumed his investigation of the liquefaction of gases. He was impressed by Caignard de la Tours' observation that if water, carbon bisulphide, and other liquids are heated in a closed tube there is a critical temperature at which they are completely vapourized, even if the pressure is very high. He saw that this implied that the liquefaction of many gases could not be accomplished by the application of any pressure above a certain critical temperature. He found that the critical temperature for carbon dioxide was about 32° C. In the course of these experiments he produced temperatures of 100° C. by pumping off carbon dioxide from freezing mixtures of solid carbon dioxide and ether. But he was unable to liquefy oxygen, nitrogen, hydrogen and certain other gases. On August 30th, 1845, when he was nearly fifty-four years old, he started, for the sixth time, to attempt to discover some connection between light and electricity. He repeated the experiments on the attempt to vary the polarization of a beam of light by passing it through an electrolyte transmitting a current. Then he tried passing the beam through rock-crystal, Iceland spar, flint glass, heavy glass, turpentine and other substances when subjected to powerful electrostatic tension by electrical machines, but was unable to observe any effect. Thirty-two years afterwards the effect was detected by Kerr with the much more sensitive apparatus available to him. After a fortnight he decided to try the effect of powerful magnetic fields. He submitted rock-crystal, flint glass and other transparent materials to the field of a powerful electro-magnet, and looked for changes in polarization and other possible effects in a transmitted beam of light. They gave no effect. Then he thought of the heavy glass he had made twenty years before out of lead boro-silicate. He laid a piece of this across the poles

of the electro-magnet and passed a beam of polarized light through it longitudinally. He found that the polarization of the beam had been affected; "thus magnetic force and light were proved to have relations to each other." Further investigation showed that the electric field rotates the plane of polarization through an angle dependent on the strength of the field and its direction. He inquired whether magnetism could make a thin film of iron transparent, a phenomenon subsequently discovered by Kundt. Then he began to look for a converse effect. If magnetic force could affect light, perhaps light could produce a magnetic or electric effect. Subsequently, electrical effects due to light were discovered by Mayhew, Becquerel and others. In 1878 Becquerel showed that the plane of the polarization of the light from the sky is rotated by the earth's magnetic field. Before the end of 1845 he made another first-class discovery, again with the help of his heavy borate-of-lead glass. He noticed that "if a square bar of this substance, of about half an inch thick and two inches long, be very freely suspended between the poles of a very powerful horse-shoe electro-magnet, immediately that the magnet force is developed the bar points; but it does not point from pole to pole, but equatorially or across the magnetic lines of force." He then tried other substances and found that reaction to a magnetic field was a universal property. All substances were orientated either along or across the lines of magnetic force. He named substances which were orientated along the lines "paramagnetic"; while those that were set across were named "diamagnetic." He found that copper, bismuth and phosphorus were noticeably diamagnetic, and paper, sealing-wax, beef, apple and bread also were diamagnetic. "If a man could be suspended with sufficient delicacy after the manner of Dufay and placed in the magnetic field he would point equatorially."

The repulsion of bismuth by a magnet had been observed before by Brugmans, and by Le Baillif, but they had not explored the phenomenon.

Faraday was now aware of four general aspects of electro-magnetic phenomena, three of which he had discovered

himself. First, there was Ørsted's demonstration that electricity in motion produces magnetic force. Second, there was his own discovery that a change in magnetic force produces an electric current. Third, his discovery that electric forces affect dielectrics; and fourth, his discovery that magnetic forces affect dielectrics. His conception of lines of force, bearing longitudinal and transverse tensions, gave a picture of the mode of operation of electro-magnetic forces, and his experimental measurements confirmed simple calculations of electro-magnetic effects deduced from the mechanism of this picture. Kelvin showed that this mechanism was susceptible of mathematical description, and from this suggestion Clerk Maxwell made his description, with results that will be described in another chapter.

But Faraday himself had something to say about his picture. Early in 1846, before he had discovered the magnetic polarization of light and diamagnetism, he had to lecture "suddenly and occupy the place of another." Not having a fully prepared lecture ready, he filled the latter part with some extempore "Thoughts on Ray-vibrations." "The point intended to be set forth for the consideration of the hearers was, whether it was not possible that the vibrations which in a certain theory are assumed to account for radiation and radiant phenomena may not occur in the lines of force which connect particles and consequently masses of matter together; a notion which, as far as it is admitted, will dispense with the ether which, in another view, is supposed to be a medium in which these vibrations take place." He refers to his conception of particles as concentrations in fields of force, an idea also held by Boscovitch, which has already been quoted. According to this conception a particle has no definite size: "the particle indeed is supposed to exist only by these forces, and where they are it is. The consideration of matter under this view gradually led me to look at the lines of force as being perhaps the seat of the vibrations of radiant phenomena." He remarks that the velocity of light and electricity are known to be approximately the same. He explains that a line of

force which ends on a vibrating particle might be set in a vibrating motion by the vibrating particle, and that a line of gravitational force or a line of magnetic force might be an effective vibratory agent of this sort. "The view which I am so bold as to put forth considers, then, radiation as a high species of vibration in the lines of force, which are known to connect particles and also masses of matter together. It endeavours to dismiss the ether, but not the vibrations." He believes that the phenomena of polarization can be explained only by a lateral vibration along a line of force. In 1864, in his paper on a *Dynamical Theory of the Electro-magnetic Field*, Clerk Maxwell wrote: "The conception of the propagation of transverse magnetic disturbances to the exclusion of normal ones is distinctly set forth by Professor Faraday in his *Thoughts on Ray-vibrations*. The electro-magnetic theory of light, as proposed by him, is the same in substance as that which I have begun to develop in this paper, except that in 1846 there were no data to calculate the velocity of propagation."

In 1846 Faraday had suggested the electro-magnetic theory of light, and was doubtful of the existence of the ether. During the next twenty-one years he added a number of new facts and considerations to these suggestions. He made interesting discoveries such as the phenomenon of the regelation of ice, the property by which ice can solidfy water in contact with it. This phenomenon explains why snow-balls can be made only with damp and not with dry snow, and has a part in the behaviour of glaciers. In 1853 he demonstrated that certain table-turners could not turn the table if it was separated from their hands by rollers or lubricated surfaces. He would not consent to investigate a subject without examining it thoroughly, and when he had concluded that the phenomena were due to "a quasi-involuntary muscular action," he would never consent to investigate mediums again. He was always looking for unknown forces. He often tried the "passing of hands" and other unorthodox methods on physical experiments, in the hope that he might discover some fundamental new power; but without success.

In 1856 he gave the Bakerian Lecture on the *Experimental Relation of Gold (and other Metals) to Light.* This was his last contribution to the Transactions of the Royal Society. He described the production of a "ruby fluid" by treating solutions of gold chloride with solid phosphorus, and other substances. He observed that it was very sensitive to the addition of small quantities of electrolytes, and that the sensitivity could be reduced by the addition of jelly. He concluded that the ruby fluid contained "no dissolved, but only diffused, gold." He attributed the colour of gold-ruby glass also to the diffusion of small particles of the metal in the glass. The importance of Faraday's observations for the conception of the particles of colloidal substances was not fully appreciated until about forty years later.

In 1857 Faraday refused to accept the Presidency of the Royal Society. He had not been happy about its constitution and management, and now he also did not feel strong enough to bear the mental strain of the administrative duties.

By the authority of the Prince Consort he was provided in 1858 with a comfortable house near Hampton Court, for the rest of his life. He gave his last course of juvenile lectures in 1860, and in 1861, at the age of seventy, resigned from his professorship at the Royal Institution. His last research was made on March 12th, 1862. He looked for evidence for the refraction of a beam of light by a magnetic field. The final entry in his note-book is: "Not the slightest effect on the polarized or unpolarized ray was observed." In 1897 the effect was discovered by Zeeman. Faraday retired from the field of experimental investigation in an attempt to make one of the experiments which has proved most fundamental for twentieth-century physics. At the age of seventy he was still pursuing eternal visions.

No genius whose works have been described in history was discontented more divinely than Faraday with the narrow boundaries of human accomplishment.

During the last years of his life, his physical and mental powers gently decayed, and he died in 1867, at the age of seventy-five years.

Faraday's magnitude may be appreciated by comparison with genius in other fields of mental achievement. It is interesting to compare him with Tolstoy. Faraday was the greatest physical scientist of the nineteenth century, and many would say that Tolstoy was the greatest writer of the nineteenth century. Both of them were men of small stature and extraordinary energy, and both had peculiar creeds with ideals of extreme self-repression. Why were Faraday and Darwin so intellectually superior to the English nineteenth-century writers, and why were Tolstoy and Dostoyevsky so superior to the Russian nineteenth-century scientists? The soul of British industrialism was expressed in its struggle to subject natural forces to commercial ends. Its voice was heard through the investigators of natural phenomena, whose researches provided the knowledge essential to such an object. The society of nineteenth-century Russia, unlike that of England, had not been launched by a dominant class towards the accomplishment of a specific object. It was in a condition of unmanageable conflict. Men of powerful intellect could find no obvious channel for the use of their energy, so they turned on themselves and wrote a literature that was as great as the conflicts that disorganized their minds. Davy and Faraday expressed the intellectual aspect of a movement that has produced the civilization based on applied science, whereas Tolstoy and Dostoyevsky expressed the intellectual aspect of a situation that has produced a communist organization of society. Scientific civilization and communism are two of the major human ideas. Faraday's and Tolstoy's intimate relation to them is the explanation of their greatness, and one of the explanations why the works of their respective literary and scientific contemporaries were of a lower order of value.

It has been remarked that in his extant photographs, Faraday looks like an American. This comment suggests interesting comparisons. His extreme fundamentalist creed, combined with the dominant belief in the value of experiment, would have made him more comfortable than any of his English intellectual peers in the American social

environment. The most tremendous applications of his discoveries, such as electrical engineering, have been made in America. Faraday's American qualities were partly due to his proletarian origin. Like Davy, he did not have the university education which was the mark of the educated member of the English governing class. Davy and Faraday were the forerunners of the middle-class scientists who did not capture the English universities until the middle of the nineteenth century. Faraday resembled Priestley and other Puritans who were excluded from the English universities and had to find freedom in America.

The social origin of Davy and Faraday had remarkable directly scientific effects. In the whole of their glorious works there is not a single algebraical or chemical formula. They were entirely outside the mathematical tradition of the universities of the English governing classes. This tradition was founded before industrialism had become dominant, and was not assimilated to the needs of industrialism until the middle of the nineteenth century. The sociological meaning of the apparition of Kelvin and Clerk Maxwell at that date is that the British industrial *bourgeoisie* had captured the ancient universities. The achievements of Davy and Faraday with experiment, imagination and the rule-of-three, is extremely impressive. They imply, among other implications, that the importance of mathematics may often be secondary, and is dependent on the correct conception and imaginative interpretation of the primitive facts. In this connexion, it is interesting to note that Newton's *Principia* contains no algebraical formula. He secured acceptance for his revolutionary ideas by expressing them in the scientific language of the preceding cultural period. Faraday's ideas were not accepted until Maxwell expressed them in the mathematical terminology of the preceding scientific period.

Before taking leave, for this occasion, of Faraday, a summary of his chief discoveries and their influence, may be made. His greatest achievement was the construction of a complete descriptive theory of electricity. This included the invention of the electro-magnetic theory of

light, and provided the most important starting-point for the theory of relativity. It is difficult to arrange his marvellous experimental discoveries in any order of merit. His discovery of electro-magnetic induction is usually given the first place. This made modern electrical engineering possible. He invented the transformer (indeed, he discovered electro-magnetic induction with the first transformer) and the dynamo. The telephone is a direct application of electro-magnetic induction. The radio is an invention which arose out of the development of his theory of electro-magnetism. His investigation and theory of electrolysis is the chief contribution towards the scientific foundation of the electro-chemical industry. He discovered benzene, the basis of the synthetic dyestuffs industry. He invented the instrument which contains the principle of the cinematograph. Besides these he made a dozen discoveries any one of which would have caused his name to be remembered in the history of science. His work as a whole transcends the boundaries of the history of science and has become a feature in human history.

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III

JAMES PRESCOTT JOULE

1818–1889

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JOULE is one of the most peculiar figures in the history of science. His quality as an experimenter has never been surpassed. The rapid maturity of his powers, his intellectual independence and clarity, and theoretical insight into the significance of his experiments might have made him an immediately fascinating personality. But he seems to inspire respect rather than excitement. The student usually learns that he was a great experimenter who determined the mechanical equivalent of heat, and established the principle of the conservation of energy, the chief contribution of the nineteenth century to physical science. An account of the measurement of the rise of temperature produced in water by rotating paddles is followed by comments on the comprehensive nature and importance of the principle of the conservation of energy. The student tries to be properly impressed, and passes on to something less important, but apparently more interesting. What is the explanation of this tendency to admire rather than respond to Joule's extraordinary achievements?

His retiring disposition and the cultural barbarity of his environment are among the factors that have prevented Joule and his researches from having an expected fascination. His personality was not powerful enough to overcome the uncivilized darkness with which he was surrounded. If society with educated imagination had existed in Manchester, Joule's discoveries might have been the material of

a magnificent literature. He could make powerful imaginative deductions from his results, but only of a direct sort. He did not notably possess the power of applying a new idea through various regions of phenomena. The idea of the convervation of energy, and a calculation of the mechanical equivalent of heat, were published by J. R. Mayer, in Germany, before Joule had published similar results. Mayer was the intellectual complement of Joule, as he possessed a power of imaginative generalization equal in degree to Joule's experimental power. In their different styles they were men of equal genius. Both of them, in spite of their achievements, failed to stimulate the right sort of cultural response in their contemporaries. This prevented the composition of a literature that explained the full interest of their work. The scientific work of Joule and of Darwin have certain resemblances in qualities. Their work was baldly scientific and the results were of vast importance, but neither was concerned with the explanation of the general implications. Joule's papers are in fact less tedious to the general reader than many of Darwin's. Besides the nearness of Darwin's chief theme to the personal interests of humanity, he had the advantage of having its implications explained by T. H. Huxley, and Huxley's pupil, H. G. Wells. Joule was interpreted by William Thomson, whose original genius was coarser than Joule's, and who lacked the mastery of speech, and the insight into human nature, that made Huxley such a brilliant expositor. Thomson was seconded by P. G. Tait, a man of fine talent and high personal character, but lacking in philosophic depth. Tait conducted scientific controversies like football matches between teams of schoolboys. He was loved by his pupils, but the magnitude of the affection he inspired in them does not excuse the limitations of his personality. His patriotic and social narrowness seems to have been a product of the security of the British professional class and of Britain. His limitations seem to have been due to circumstances that his gifts did not enable him to overcome. He had not sufficient insight. Some of Thomson's limitations seem to have been of a less pardonable sort.

It is possible that the theory of the conservation of energy is intrinsically less interesting than the theory of evolution. Its discovery is not attributed exclusively to Joule, as the discovery of evolution is so often attributed to Darwin, and its implications were not explained by a Huxley and a Wells. The inability of Joule and his circle to interest the public in the theory of the conservation of energy may have been connected with an unconscious resentment against physical science. Manchester was one of the centres of the barbarous industrialism based on the application of the steam-engine to manufacture. The subtler activities of the human intellect may have been repressed in the face of the brutal pursuit of wealth that accompanied the development of production by the application of physical science. The comprehensive human imagination could not be nourished by Joule's discoveries because they sprang from poisoned social sources. They arose out of studies of engines that had been appropriated to the creation of private wealth instead of an increase of human dignity. Much of the popular interest in the theory of evolution was due to the convenience of the principle of natural selection to the philosophy of competitive industrialism. Theories of evolution had frequently been propounded before Darwin, but his theory secured recognition partly because it combined evolution with natural selection, and the idea of natural selection was popular for other than scientific reasons. It is possible that Darwin's researches are not intrinsically more interesting than Joule's, in spite of their vastly greater literary success, and that the difference in the effects inspired by them is partly due to the more direct connexion of Joule's researches with the sordid aspects of machine industry. Problems of horse-power were closely connected with those of production and the pursuit of wealth, whereas the survival of the fittest seemed to be connected with the sublimer problems of life and death, though the phrase's fascination derived its power not from these problems, but from its apparent justification of the methods of competitive industry.

Joule had no comprehensive imagination, so the failure

to present the comprehensive implications of his work was due to the failure of his colleagues. Mayer possessed the highest degree of comprehensive imagination, but his work was not understood by his colleagues. The mode of his discovery of the principle of the conservation of energy and of the mechanical equivalent of heat was extremely ingenious, but even this was insufficient to stimulate contemporary imagination. The apparent inexplicable boredom with which so many students approach the brilliant researches of Mayer and Joule is partly due to the blight which has infected culture since the heat-engine was harnessed to the pursuit of private wealth instead of social improvement.

Joule was born on December 24th, 1818, at Salford, which is adjacent to Manchester. His ancestors were Derbyshire yeomen or small farmers. His grandfather had migrated to Salford, where he founded a brewery and became wealthy. The grandfather's family, including his fourth son, Benjamin, conducted the business after he died. Benjamin Joule had five children, of which James Prescott Joule was the second. The eldest was Benjamin St. J. B. Joule. The ownership of the brewery passed to Joule's father. He sold it in 1854, and died in 1858, at the age of 74. Thus the Salford brewery passed out of the family when James Prescott Joule was thirty-five years of age. He never had an active part in the management of it. Joule's father was interested in politics, on the Conservative side. His eldest son acted for a time as his political agent. Joule also was a political Conservative, and he was very conservative, too, in his attitude towards affairs. When he was President of the Manchester Literary and Philosophical Society he nearly always opposed any modification of the traditional procedure. Joule's conservatism in affairs is in remarkable contrast with his radicalism in research. His early papers show an independence of authority unsurpassed in any great investigation by a young man under the age of twenty-five.

The Joule family do not seem to have been physically robust. His mother died in 1834, at the age of forty-eight,

and his father was an invalid for the last nine years of his life. His younger brother died at the age of about forty-five. For the last twenty-five years of his life Joule appears to have suffered from nose-bleeding, presumably hæmophilia. He was slightly deformed and in his youth was treated for spinal trouble. This does not seem to have been very serious as he was then physically active. Perhaps his later shyness and his general conservatism in affairs was increased by physical delicacy.

Joule and his elder brother were educated at their father's house, Broom Hill, near Manchester. Like Davy, Joule received a very early impression of the machinery of the new industrialism. At the age of fifteen Joule began to do some work in the brewery in order to learn the business. A brewery contains many processes of interest to the scientific mind. The evolution of carbon dioxide raises questions of the nature and properties of gases other than air. Joseph Priestley started his classical experiments on gases by investigating the carbon dioxide obtained from a brewery. The science of bacteriology arose out of Pasteur's studies of the problems of the French wine industry. Much wine was unaccountably being spoilt and he was asked to investigate why. In recent decades research of fundamental importance on the nature of acidity and its part in biological processes has been done by Sørensen of the Carlsberg Laboratories at Copenhagen. His studies of fermentation have led to the theory of hydrogen ion concentration.

The brewer must know something of the chemistry and physics of gases and liquids. He must be a sufficiently good bacteriologist to prevent his fermentations from being spoilt. He must know enough of engineering to be able to handle and pump large quantities of liquids and gases at various temperatures.

This third part of the brewer's technique probably impressed Joule. His insight into the relation of the temperatures and pressures of gases, into heating apparatus, and pumping engines may have begun to grow during the hours he spent in the brewery in his boyhood. Davy

played among the pumping machinery of mines, Joule among that of a brewery. When Joule's father decided to send Joule and his brother to John Dalton for lessons in chemistry twice a week, he probably thought more of providing his sons with scientific knowledge of industrial value, than of educating their spirits through the study of science.

There were other circumstances that could have stimulated Joule's interest in mechanism and science.

His brother's diary contains an account of their expedition, on September 15th, 1830, "into a field near Eccles, to see the first trains which travelled between Liverpool and Manchester, and to their riding on several Saturday afternoons to a place between Eccles and Patricroft to watch the two trains (one on each set of rails) passing and repassing for the amusement of passengers to Newton-in-the-Willows and back."

The early education of Joule was started by his half-sister, and continued, with that of his brother, by resident tutors. One of these was F. Tappenden, who came from a military school in the south, and remained with them from 1832 to 1834. They rode ponies and were already interested in scientific toys, as they passed electric shocks through friends and servants while arranging that the current should in appearance, though not in reality, pass through themselves. They repeated Franklin's experiment on bringing down electricity by kites, and verified its dangers.

According to J. T. Bottomley, Joule's first electrical machine was of the glass cylinder type. It contained a poker hung up by silk threads. Leyden jars consisted of bottles half-full of water standing in another vessel filled with water.

In 1834 their father decided to send them to study chemistry under John Dalton. The famous philosopher was then sixty-eight years old, and still, to the eternal discredit of his contemporaries, earning pocket-money by teaching children. He insisted that his pupils should have a good knowledge of arithmetic and the first book of Euclid before beginning chemistry, so the young Joules

were specially prepared in these subjects by their tutor, Tappenden, before they went to Dalton. Even after this the pupils spent the first two years in two weekly lessons of one hour covering the same ground. They were not pleased with this, and when Dalton suggested they should proceed to the higher mathematics before starting chemistry, they declined. It is interesting to note the leisurely approach of the founder of modern chemistry to laboratory experiment in the teaching of chemistry, and his emphasis on the importance of mathematics. Through this attitude the Joules received little instruction in chemistry from Dalton, as his course was ended in 1837, owing to a severe attack of paralysis.

J. T. Bottomley writes that Dalton taught the Joules arithmetic, algebra and geometry, and then natural philosophy from Cavallo's textbook, and then chemistry from his own *New System of Chemical Philosophy*.

It is possible that in this short period of instruction and contact Dalton communicated or strengthened in Joule intellectual attitudes of imperishable value. Dalton had established the atomic theory of chemistry by introducing systematic measurement and quantitative comparison into the investigation of the chemically equivalent weights of elementary substances, and abiding by the implications of measurements. As Osborne Reynolds has remarked, the chief distinction between Dalton and Joule and their early contemporaries was the same, namely, the substitution of quantitative measurement for phenomenal experiments. Joule arrived at the law of the conservation of energy through systematically measuring several physical and mechanical effects, and comparing them with the equivalent electrical effect. The works of Dalton and Joule are both distinguished by the emphasis on measurement, and independent reliance on its results. The relation of Joule to Dalton is inevitably compared with that of Faraday to Davy. All of these men received no higher education in the ordinary sense apart from self-instruction. Faraday acquired the technique of research during long years of daily labour with Davy, and had scarcely a thought of making an independent

research until he was twenty-five. Joule had wonderful gifts of construction and manipulation which required little training, but it is possible that he was deeply indebted to Dalton for the independence of his intellectual attitude. Dalton evidently liked the young Joules, as they noted after calling on him in 1838 that he "seemed very pleased to see us."

Another profoundly important source of Joule's intellectual independence was his financial independence. As a rich young man he needed no conventional training to qualify him for a career, or introduce him to powerful future friends. His early researches were pursued partly in the spirit of a young gentleman's entertainment, which happened to be science instead of fighting or politics or gambling. It is difficult to believe that any student who had received a lengthy academic training could have described researches in Joule's tone of intellectual equality. The gifted student who had studied under a great teacher would almost certainly adopt a less independent tone in his first papers, because he would have the attitude of a pupil to his seniors, besides a deference due to appreciation of his senior's achievements. A student without deference after distinguished tuition is almost always mediocre.

Some very valuable material concerning Joule was acquired by the late Professor W. W. Haldane Gee on behalf of the Manchester College of Technology. It was found in the cellars of Joule's last place of residence, at 12 Wardle Road, Sale. The writer is indebted to the authorities of the Manchester College of Technology for the permission to examine and comment on this remarkable collection. The pieces of apparatus include the pump, receivers, and double-walled calorimeter used in the famous experiments, described later in this chapter, in which Joule proved that gases which expand without doing external work do not sensibly change in temperature. There are cores of two of the electro-magnets used in his experiments of 1839, a travelling microscope used in the callibration of his thermometers, and many other smaller items.

The note-books and manuscript notes are still more interesting. There are six laboratory note-books that contain Joule's original accounts of all of his experiments done between the years 1839–1871.

The first note-book was used between 1839–43 and contains 260 pages. It was one of the old exercise-books from his boyhood. There are several pages of exercises in book-keeping. The real or imaginary accounts are dated 1825. Then there are more pages of exercises in arithmetic and commercial arithmetic, and a few pages on the properties of conic sections. All of these exercises are written in a copper-plate handwriting. Gee has suggested they were done under the superintendence of a tutor as a preparation for Dalton's instruction. But Reynolds writes that Dalton disappointed the young Joules by keeping them to very elementary work. Perhaps these exercises were done under Dalton's own eye.

When Joule began original experiments as a youth he used this and other unfilled exercise-books as laboratory note-books. His first results of experiments, on electro-magnets, are dated 1839, and their scratchy untidiness is in contrast with the neatness of his childhood exercises. The second note-book contains 434 pages and was used between 1843–58. The third note-book contains 180 pages and is dated 1885–1871. A fourth contains 154 pages of notes and drafts of published papers, including that of his famous lecture in the reading-room of St. Ann's Church, in Manchester, his first public statement of the law of the conservation of energy, and some of its consequences. A fifth and sixth volume contain 138 and 144 pages of "papers in rough" and drafts. The sixth volume contains a translation of Mayer's paper on the *Forces of Inorganic Nature*, and other foreign papers. There are pencilled marks and a few sentences commenting on the assumptions in Mayer's method of calculating the mechanical equivalent of heat. The translation is undated. Joule may have copied the version from that published in the *Philosophical Magazine* in 1862. The book contains entries written at dates between 1845 and 1877.

The aspect of these six note-books raises several interesting suggestions. It is astonishing that most of the life-work of a genius should have been accommodated in six comparatively small books. Apparently Joule could write the description of his great discoveries straight down, almost without correction. This seems to show he had a mind of extraordinary clarity. He was never groping. He did not fill thousands of pages with false starts and records of incorrect observations as he advanced towards the truth. The use of these old school-books, written from both ends, shows an extraordinary lack of formality. He did not buy special note-books to record the great discoveries. That he rarely had to revise the figures of his observations, and that he recorded the precious new knowledge in the first note-book that came to his hand shows that his mind naturally worked at an extraordinarily high standard. His experiments were not surrounded by vast quantities of scaffolding that disappeared before the announcement of the result. He did not separate a quantity of truth from a large number of groping, unsuccessful experiments. Nearly all of his experiments seem to have been perfectly conceived and executed, and the first draft of the description of them could be sent almost without revision to the journals for publication.

In addition to the note-books there are some loose sheets of paper whose contents are described by Dr. H. Lowery as "rough notes on the scope and methods of physical science," and "the aims of science and the true spirit of research."

The writer believes these notes may be the draft of the address Joule was to deliver in 1873, as President of the British Association meeting at Bradford. Owing to ill-health Joule had to resign the Presidency, so he did not deliver the address.

Joule writes that natural philosophy is second only to religion. "After the knowledge of, and obedience to, the will of God, the next aim must be to know something of His attributes of wisdom, power and goodness as evidenced by His handiwork."

"The study of nature and her laws" is "essentially a holy

undertaking" and is of "great importance and absolute necessity in the education of youth."

The pursuit of science is due to "a love of wisdom which it unfolds, a love of truth for its own sake independently of a regard to the advantages of whatever kind which are expected to be derived from it." It is inspired by "a certain inquisitiveness of mind to know that which is already unknown, a principle which is one of the most important that belongs to our nature and is in fact the principal cause which reconciles us to life which would be miserable indeed if it presented merely the recurrence of the same objects and gave no hope of varied and fresh enjoyments."

The scientist must be humble, diligent, energetic, patient and zealous. He should "receive a healthy stimulus from a well-regulated love of approbation, and the hope of success."

The first object of natural science is to elevate humanity in the scale of creatures, and the second is to promote well-being.

"The first object is therefore at least as much more important than the second as the intellect is more noble than the body."

"It is evident that an acquaintance with natural laws means no less than an acquaintance with the mind of God therein expressed."

Concerning applied science, "Nothing is more expensive than the endeavour to stumble blindly on an improvement or an invention."

Speaking of the lack of scientific knowledge in the designers of the early submarine cables, he remarks that such knowledge "ought to be possessed by a gentleman in whose position one would certainly require; what is however seldom found, as accurate knowledge of physical science as is commonly possessed by a joiner or a mechanic."

He pleads for education in science, and the communication of science to the public. Man without science is half-educated. "In the first place we may remark that the trite proverb that a little knowledge is a dangerous thing, absurd in other cases, is peculiarly so in this. This doctrine of

fools would necessarily discountenance any education whatsoever because in passing from ignorance to the highest state of intellectual acquirement a man must be at one time induced with the dreaded little knowledge. The truth is that a little knowledge is a little good and much knowledge is a great good, while ignorance is an unmitigated evil allying us with the beasts that perish."

Mathematics is of great importance to the scientist, but even advanced workers should frequently return to the simple truths. The analyst must continually recur to the descriptive facts from which he started.

Joule includes one passage with a tone remarkably familiar to our own days of 1935. "Such then are the legitimate objects of science. It is deeply to be regretted that another and most unworthy object has been introduced and has gradually and alarmingly increased in prominence. This is the improvement of the art of war and the implements of mutual destruction. I know there are those who think that these improvements will tend to put an end to war by making it more destructive. I cannot think that such an opinion is based on common sense. I believe war will not only be more destructive, but be carried on with greater ferocity. Individual campaigns will doubtless be short as well as decisive, but this will necessarily cause that rapid rise and fall of states and unsettling of boundaries and constitutions which must eventually deteriorate civilization itself and render peace impossible. And thus by applying itself to an improper object science may eventually fall by its own hand. In reference to this subject we must also deplore the prostitution of science for the aggrandisement of individuals and nations, the result being that the weaker is destroyed and the stronger race is established on its ruins. In making the above remarks I allude to war generally, I intend no disparagement of the effects being made to secure the integrity and liberties of Great Britain. These have been forced upon us and it is matter of congratulation that we are not responsible for the present military attitude of Europe."

After considering these expressions of his mature and experienced mind, which are so acute and in their political

application so characteristically British, it is necessary to return to a description of his youth.

The young Joules were fond of life out of doors. They rode over the country-side and climbed the hills in the Lake District and in the Pennines. On June 8th, 1838, they rowed to Lowood Inn by Lake Windermere and back before breakfast, and ascended Helvellyn from Wyburn, and played at snow-balling on the summit. On June 12th they raced and beat two of the best rowers in the district in the morning, and in the afternoon climbed Skiddaw. They were very fond of pistols and guns, and had special small-bore designs made for them. They combined this vigorous life of amusement in the fresh air with meteorological observations, which they reported to Dalton. On one occasion they noted thunder that came from a distant storm, and counted the seconds between the flashes and the sound. Dalton told them that they had probably heard a great storm at Holyhead, forty miles away, and that in his own experience he had never heard a sound from so distant a source. In 1840 James experimented on a lame horse with galvanic electricity. In 1842 he was firing pistols on Lake Windermere in order to observe the echo. His brother was startled by a tremendous report and when he turned round he found that James' pistol had jumped out of his hand into the lake. He had stuffed three times the usual charge into the pistol in order to produce a loud noise and a fine echo. At another time he shot off his own eye-brows. The brothers made soundings all over the lake and discovered the maximum depth to be thirty-three fathoms.

Their interests were not confined to athletic exercise and scientific observation and experiment. The elder brother was an enthusiastic musician and James had much skill in painting and photography. He collected pictures and paid as much as £50 for a cattle piece. This sum is an effective illustration of the conditions of Joule's early life. At that date £1 per week was a high wage for a workman. The young man could afford to buy pictures at a price equal to the year's income of a skilled workman.

When he was older Benjamin Joule cultivated church

music. He was the author of *A collection of Words of 2,270 Anthems* and other musical works. He was virtually the founder of the Manchester Vocal Society, and for many years arranged the musical services in St. Peter's Church, Mosley Street, Manchester. He made the organ the finest in the city, and large attendances were attracted to the gloomy church by the beautiful music. He wrote the music criticisms for the *Manchester Courier* for over twenty years, and his connection with that paper provided an incident of importance to science, as will be related presently.

In the 1890's it is reported that he had an "old-world flavour in his speech." He was Conservative, but not very keen on party, and an enthusiastic antiquary.

In 1838 Joule turned one of the rooms in his father's house into a laboratory, and started unconsciously the researches that ended in his great discoveries. His first short paper was published in the same year, when he was nineteen years of age. He became acquainted with the scientists who met in the Manchester Literary and Philosophical Society, and often called on Dalton and stayed to tea. In 1839 he received private lessons in chemistry from John Davies, and by 1842, Davies, Ransome and Sturgeon, the inventor of the electro-magnet, were being entertained to dinner at Broom Hill.

The father of Joule, like the father of Clerk Maxwell, deserves much credit for his provision of conditions in which the talents of his son could unfold.

Lyon Playfair, the chemist and politician, was professor of chemistry in the Royal Institution of Manchester from 1842–45 and published papers in collaboration with Joule. In a recollection of this period he wrote that Dalton, who died in 1844, "naturally gave impulse to the study of science in the town, where there was an active band of young workers in research. Joule was even then the foremost."

Liebig and Bunsen visited Playfair in 1845 and he introduced them to Joule, who, he writes, "was a man of singular simplicity and earnestness. We used to meet at each other's houses and supper to help the progress of research."

"I was anxious that he should devote his life to science, and persuaded him to become a candidate for the chair of natural philosophy at St. Andrews. He was on the point of securing this, but his personal slight deformity was an objection in the eyes of one of the electors and St. Andrews lost the glory of having one of the greatest discoverers of our age."

"Dalton and Joule used to sit side by side at the Manchester Royal Institution—the keenly intelligent Joule."

When Roscoe became professor of chemistry at Owens College in 1857, Joule's researches were the subject of his first public lecture in Manchester. He had recently come from Germany, where he had studied under Bunsen, and had learned of Joule's great reputation in Germany.

Joule's early correspondence frequently displays his precision of thought and character. The Manchester Literary and Philosophical Society possesses a letter dated July 10th, 1848, in which Joule insists that the Society should present its memoirs only to other societies that make a return. "We must be just to ourselves."

In another letter he discusses what should be done with a mathematical paper upon which the referees gave no opinion. He considered that the safest and best plan was to publish. "I believe that if we are to wait for perfect mathematical papers we may wait long enough."

On February 6th, 1849, he addressed a note from his laboratory at the Brewery, New Bailey Street, Salford:

"Will you kindly inform me whether it will be convenient for you to attend the meeting to-night. I am anxious to hear Jenny Lind, but if you intend to go or are otherwise occupied I will endeavour to be present at the Society's meeting."

Presumably he intended to go with his brother, who would write a notice of the concert for the *Courier*.

In 1851 he wrote a letter of resignation as Vice-president of the Society because its late President was excluded from its councils.

Osborne Reynolds first saw Joule in 1869. He writes:

"That Joule, who was then 51 years of age, was rather

under the medium height; that he was somewhat stout and rounded in figure; that his dress, though neat, was commonplace in the extreme, and that his attitude and movements were possessed of no natural grace, while his manner was somewhat nervous, and he possessed no great facility of speech, altogether conveyed an impression of the simplicity and utter absence of all affectation which had characterized his life; while his fine head and the reflective intelligence of his grave face accorded with the possession and long exercise of the highest philosophical powers."

He was obviously venerated by the members of the Society. He was kindly, noble and extremely chivalrous, but hated quackery, especially from persons of standing.

He encouraged the efforts of workers as yet unknown, and resented disparagement of their work, "as though his own early experience had left him with a fellow-feeling with those who were struggling" to secure recognition of their results.

The ideological origin of Joule's researches has been admirably discussed by Osborne Reynolds. His *Memoir* of Joule is perhaps the best biography of a scientist in the English language. It is one of the very few in which the source and sequence of the subject's ideas have been examined by a scientist of first-class intellect and command of expression. Much of what he writes will be followed closely here, and it is hoped that some will be prompted to read for themselves the Sixth Volume of the Fourth Series of the *Memoirs* of the Manchester Literary and Philosophical Society.

In 1838 the words "energy" and "work" did not have the same meanings as they have to-day. Young had defined "energy" as one-half of the mass of a moving object multiplied by the square of its velocity. He regarded it mainly as a mathematical function that assisted in the solution of problems in Newtonian mechanics. The theory of mechanics developed by Galileo and Newton was based on the study of the motion of comparatively unresisted objects, such as heavy bodies through air, or planets through empty space. As their formulæ had been derived

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(Engraved by C. H. Jeens. London, published by Macmillan & Co., Ltd. Nature and Physical Society)

from a study of motions that do not commonly occur on the surface of the earth they had a sort of disembodied quality, and when they happened to apply to such motions, the application was felt to be accidental rather than real. The Newtonian mechanicians were also familiar with the idea of "work" as the product of force and distance, but again mainly as a mathematical function, and they knew that this function could be equal to that of "energy." They used this mathematical equivalence for solving the problems of frictionless motion. The idea of "work" as the product of the distance multiplied by the mean resistance overcome, and hence as a fundamental measure of mechanical action, did not come from the study of celestial motions, but from entirely different sources. This was natural, because no one was interested in the "energy" or *vis viva* of the planet Mars, for example, except as a mathematical function that assisted the solution of problems of solar motions. It was not necessary to evaluate the "energy" of Mars in units because the symbols $\frac{1}{2} M v^2$ melted away in the process of mathematical transformations of equations. A knowledge of the "energy" of Mars is of no use except in the solution of mathematical problems. It has no direct financial value.

The study of motion in the factory and industry engendered an entirely different variety of terminology. The introduction of the steam engine for pumping and factory-driving gave mechanical action commercial value. The industrial notion of "work" was of something that determined the quantity of production. An exact measure of "work" was essential to the foundation of trading in mechanical power. Machine industry naturally arrived at a conception of work in terms of gallons of water raised so many feet in such and such a time, or of foot-pounds per minute. This was a tangible, saleable commodity entirely different from the abstract conception of the Newtonian "work" done by the moon when it fell through a certain distance. The "work" of Newtonian mechanics and the "work" of industrial engineers obeyed the same algebraical laws, but the constellation of other ideas respectively associated with them was profoundly different. The engineers

described as "accumulated work" what is now familiar as energy. They considered the work spent in overcoming friction as the measure of mechanical power annihilated.

Lavoisier and Laplace discussed the nature of heat in their treatise of 1780. They wrote: "Other physicists think that heat is only the result of the insensible vibrations of matter . . . heat is the living force which results from the insensible motions of a body, it is the sum of the products of the mass of each molecule by the square of its velocity. . . . We will not decide between the two preceding hypotheses; several phenomena appear favourable to the latter, such, for example, as the heat produced by the friction of two solid bodies; but there are others which are more simply explained by the first hypothesis; perhaps both of them occur at the same time."

Later in the book they accept as evident the proposition that "All variations of heat, whether real or apparent, experienced by a system of bodies, in changing their state, are produced in the inverse order when the system returns to its first condition." If the system performs external work this proposition is erroneous. Besides showing a mode of thought that developed through the mind of Carnot into one of the most powerful of theoretical conceptions, this passage apparently prompted Thomas Young to remark in his *Lecture on Natural Philosophy*, published in 1807, that "It has not perhaps been demonstrated in a single case that the quantity of heat absorbed in any phenomenon is precisely equal to the heat disengaged when the phenomenon is inverted."

While Lavoisier never entirely abandoned the kinetic theory of heat, Laplace became completely converted to the theory of caloric. Some of the most obstinate opponents of the kinetic theory, who continued to reject it long after the decisive demonstration of the mechanical equivalence of heat, had adopted their opinion from him.

In his treatise on chemistry, published in 1787, Lavoisier suggested that caloric, the imponderable fluid which imported elasticity and temperature to matter, might be identical with *vis viva*.

The theory that heat is a mode of motion is ancient, and arose from the connection between friction and heat, and from the mobility of flames. But no one became convinced, as Reynolds explains, that the "mechanical effect which the mechanical action of overcoming friction (annihilating mechanical power) imparted to the bodies, could in itself be the very imponderable fluid, measurable, indestructible and uncreatable, that constituted caloric."

Rumford observed that two horses working steadily against a frictional resistance produced heat at a steady rate. He measured the quantity of heat produced, and even compared it with the heat that would have been produced by the combustion of the food eaten by the horses, but he did not offer any explanation of the process.

Reynolds has also remarked that though the condensing steam engine had been at work for 150 years converting heat into mechanical effect, the process of its operation was deceptive, and seemed to indicate the contrary. The condenser seemed as essential to the engine as the furnace. Further, owing to the low efficiency of the engine the quantities of heat communicated to the steam by the boiler, and removed by the condenser, are both very much greater than the difference between them, part of which is turned into mechanical effect. The significant determination of this difference by experiment is very difficult, and was not accomplished until twenty years after the discovery of the principle of the conservation of energy had shown that the difference must exist.

The very low efficiency of the early steam engines even misled the subtle-minded Carnot to invent a correct theory of a reversible cycle of physical changes on the basis of an incorrect theory of heat. This was published in 1824. Reynolds writes: "The condensing engine, however, contributed to the discovery of the mechanical origin of heat, in that it led to the recognition of work as the measure of mechanical action; and to the locomotive must be attributed the birth of that philosophical interest respecting heat and work which immediately followed its general introduction. The condensing engine had not been obtrusive—it was

not generally seen unless looked for. The locomotive is obtrusive; it will be seen: and by 1842 locomotives had obtruded themselves pretty well all over Europe. They immediately took their places as objects of as much wonder and interest to the grown people who saw them for the first time as they are still to the young; demanding the attention even of philosophers who had previously studied nothing lower than the planets."

Figures from which the mechanical equivalent of heat can be roughly calculated are given in a book by M. Séguin which appeared in 1839, and is named *Étude sur L'Influence des Chemins de Fer*. Séguin was a nephew of Montgolfier, the French paper manufacturer and inventor of the balloon. He told W. R. Grove that Montgolfier believed force was indestructible, an opinion reinforced by his consideration of the principle of the hydraulic ram. Following the suggestion of the indestructibility of force, Séguin contended that power could not be derived from a mere transfer of heat. He assumed that the expanding steam in the cylinder of a steam engine did work equal to the amount of heat it lost. He attempted to measure the amount of heat lost by measuring the heat taken from the boiler and given to the condenser, but his experimental methods were inadequate. On the assumption that the subtraction of one unit of heat from water vapour at some temperature between 100° C. and 150° C., say 120° C., reduced the temperature by one degree, say to 119° C., he calculated from the figures he had published in 1839, the quantity of mechanical work obtained by a given loss of heat. As the properties of water-vapour in this range of temperature depart widely from those of a perfect gas, Séguin's method could not give an accurate result. One unit of heat reduces the temperature of a unit of water vapour only three-tenths of a degree, and not one degree. But this incorrect datum accidentally made his result nearly correct. He concluded that one gramme of water which lost one degree centigrade would provide enough work to raise 500 grammes through one metre. The correct result is 427. Thus the first material for an estimate of the mechanical equivalent of heat seems to have

arisen from a consideration of the principles of the steam-engine, in the perspective of suggestions from the inventor of balloons. Montgolfier made his balloons rise by heating the air inside them. Apart from the ideas of the indestructibility of force inspired by the hydraulic ram, he might have guessed that heat could be converted into work from the particularly direct relation between the lifting power of the balloon and the fire underneath. The hot-air balloonist's risks may have enhanced his perception, and have enabled him to realize vividly that his own body was being lifted through the agency of the heat from the fire.

The earliest estimate of the mechanical equivalent of heat was given by Sadi Carnot. As he died in 1832 it must have been made before that year, but it remained unpublished until 1878. He left no explanation of his method of calculation. His figure is equivalent to 370, compared with the correct figure of 427. Besides this, there are indications in his posthumous papers that he had discovered the Second Law of Thermodynamics. These achievements have prompted Larmor to suggest that Carnot was the most brilliant physical scientist of the nineteenth century. He died of cholera at the age of thirty-six, without the opportunity of completing his researches and adding to his wonderful invention of the theory of reversible cycles.

The influence of the steam-engine on Carnot and Séguin is clear. The other estimate of the mechanical equivalent of heat that preceded Joule's was made by J. R. Mayer, and was inspired by a remarkably different aspect of natural phenomena. Mayer was the son of an apothecary in Heilbronn. His father had the usual physical and chemical apparatus of his profession, a natural history collection and many books. This was a sympathetic environment for Mayer's genius. His father naturally educated him for a higher position in his own profession, and sent him to Tübingen to study medicine. He did not find the university courses in physics and chemistry inspiring. After finishing the course he completed his medical training by visiting hospitals in Munich, Vienna and Paris.

His first independent appointment was as doctor on a

Dutch East Indian ship bound for Java. The voyage lasted from February, 1840 until February, 1841. There were no passengers, and a crew of only twenty-eight, whose health remained good. Mayer was twenty-six years of age. The sights of a multifarious world were before his trained mind for the first time. He had little work, and plenty of scientific books to read, and no one to share or distract his intellectual interests. His voyage to Sourabaya, now so well known as an air-port on the air-route from Europe to Australia, resembles in significance and nature Darwin's voyage on the *Beagle*. Out of one came the suggestion for the biological theory of evolution, and out of the other the theory of the conservation of energy.

Mayer wrote that he "enjoyed a harmless peace of mind which disposed him by preference to scientific occupation, and allowed him to lead a pleasant life, though in narrow circumstances and far from any companions of a like taste; no day went by without interest of some kind."

He made notes of unusual phenomena, and read some physics. He meditated on the production of heat in living organisms, and on the relation between heat and friction.

In Java some of the European crew became ill, and he had to bleed them. He observed "that the blood drawn from the vein of the arm possessed, almost without exception, a surprisingly bright red colour.

"This phenomenon riveted my earnest attention. Starting from Lavoisier's theory, according to which animal heat is the result of a process of combustion, I regarded the two-fold change of colour which the blood undergoes in the capillaries as a sensible sign—as the visible indication—of an oxidation going on in the blood. In order that the human body may be kept at a uniform temperature the *development* of heat within it must bear a quantitative relation to the heat *which it loses*—a relation, that is, to the temperature of the surrounding medium; and hence both the production of heat, and the process of oxidation, as well as the *difference in colour of the two kinds of blood*, must be on the whole less in the torrid zones than in colder regions."

This phenomenon prompted Mayer to make a critical examination of the physiological theory of combustion. Some wonderful experiments on the production of animal heat, and its relation to respiration, had been made by Lavoisier. In 1780 Lavoisier and Laplace described experiments by which the production of heat by a guinea-pig was compared with the production of heat by burning charcoal. The guinea-pig was put in an ice-box. Its production of heat was measured by the melting of the ice. The amount of carbon in the respired carbon dioxide could be determined by chemical analysis. An equal amount of carbon could be burned in the box, in another experiment, and the quantity of ice melted could be measured. They found that the guinea-pig and the charcoal produced the same amount of heat for the same consumptions of oxygen, and concluded that "respiration is therefore a combustion, very slow certainly, but perfectly similar to that of carbon."

In 1790 Lavoisier described in a letter to Black the results of experiments that will remain among the most wonderful in the history of science. He wrote that:

(I) A man at rest in a room at 26° C. consumes about 24,000 cubic centimetres of oxygen per hour.

(II) At 12° C. he consumes 28,000 c.c. of oxygen per hour.

(III) During digestion the consumption rises to 36,000–38,000 c.c. per hour.

(IV) During exercise to about 80,000 per hour.

Later investigations have proved these figures are nearly correct. The measurements are difficult and Lavoisier left no account of his methods.

In the guinea-pig experiment it may be presumed that the animal skipped about in the ice-box in order to try to get out, and to "keep warm." But the amount of ice melted was independent of the animal's mechanical movements, and dependent only on the consumption of oxygen. Hence the heat produced by the athletic movement of the animal and by friction against the side of the box must have been initially supplied by the consumption of oxygen in respiration. This heat must have been converted into animal

movement, and the animal movement back again into heat.

In the human respiration experiments the consumption of oxygen, which must lead to proportional production of heat according to the law of combustion, increases with the degree of activity. During exercise it is three times the figure during rest. Yet the temperature of the human body remains constant. What has happened to the tripled production of heat? It has been transformed into the mechanical work performed during exercise.

Mayer writes: "on the (physiological) combustion theory there is, then, no alternative, short of sacrificing the theory itself, but to admit that the *total* amount of heat evolved by the organism, partly directly, and partly indirectly by mechanical action, corresponds quantitatively or is equal to the amount of combustion."

He arrived at the idea of the equivalence of heat and work from a consideration of the human body as a heat engine. The human heat engine is particularly suitable for philosophical consideration because it has the peculiar property of running at a constant temperature. The change in temperature of the steam in steam-engines was philosophically confusing.

After perceiving that heat and mechanical work must be equivalent, Mayer began to search for examples in all regions of natural phenomena. He noticed that the amount of heat produced by compression of air might be equivalent to the amount of work done in the compression. On the assumption that the whole of the work done in compression appears as heat in the air, and that no heat is absorbed in overcoming repulsive forces between the molecules, it is possible to calculate the mechanical equivalent of heat from the ratio of the specific heats of air at constant pressure and constant volume. Mayer made the calculation. In order to establish his priority in the discovery of the conservation of energy and the mechanical equivalent of heat he wrote a short paper and submitted it to Liebig for his *Annalen der Chemie und Pharmacie*. It was published in 1842. Mayer did not mention the physiological origin of his ideas

in this paper, but expressed his theory in the philosophical terms of the equivalence of cause and effect, and in an analysis of the principles of mechanics. He remarks that "an attempt to ascertain the effects of ceasing motion has never yet been seriously made." He says that "the friction of two metallic plates continued for a very long time" can "gradually cause the cessation of an immense quantity of movement; but would it ever occur to us to look for even the slightest trace of the force which has disappeared in the metallic dust that we could collect, and to try to regain it thence? We repeat, the motion cannot have been annihilated; and contrary, or positive and negative, motions cannot be regarded as $=0$, any more than contrary motions can come out of nothing, or a weight raise itself. Without the recognition of a causal connexion between motion and heat, it is just as difficult to explain the production of heat as it is to give any account, of the motion that disappears."

In the next sentences he says that "water undergoes, as was found by the author, a rise of temperature when violently shaken. The water so heated (from 12° to 13° C.) has a greater bulk after being shaken than it had before." He supposes the expansion is due to the increased motion of the constituent molecules.

"If it be now considered as established that in many cases no other effect of motion can be traced except heat . . . we prefer the assumption that heat proceeds from motion to the assumption of a cause without effect . . . just as the chemist instead of allowing oxygen and hydrogen to disappear without further investigation, and water to be produced in some inexplicable manner, establishes a connexion between oxygen and hydrogen on the one hand and water on the other."

Here Mayer is appealing for the equivalence of work and energy by analogy with the chemical law of the conservation of mass.

"In water-mills the continual diminution in bulk which the earth undergoes, owing to the fall of water, gives rise to motion which afterwards disappears again, calling forth unceasingly a great quantity of heat."

It is interesting to note that workshops were sometimes heated by iron discs rotated against a brake by a belt from a water-wheel.

Mayer writes that "a locomotive engine with its train may be compared to a distilling apparatus; the heat applied under the boiler passes off as a motion, and this is deposited again as heat at the axles of the wheels."

"If falling force and motion are equivalent to heat, heat must also naturally be equivalent to motion and falling force."

"By applying the principles which have been set forth to the relations subsisting between the temperature and the volume of gases, we find that the sinking of a mercury column by which a gas is compressed is equivalent to the quantity of heat set free by the compression; and hence it follows the ratio of the capacity for heat of air under constant pressure and its capacity under constant volume being taken as $=1{\cdot}421$, that the warming of a given weight of water from 0° to 1° C. corresponds to the fall of an equal weight from the height of about 365 metres. If we compare with this result the working of our best steam-engines we see how small a part only of the heat applied under the boiler is really transformed into motion or the raising of weights; and this may serve as justification for the attempts at the profitable production of motion by some other method than the expenditure of the chemical difference between carbon and oxygen—more particularly by the transformation into motion of electricity obtained by chemical means." These points from Mayer's preliminary paper illustrate the brilliance of his insight. His later papers contain numerous original suggestions of the highest power, such as the effect of tidal friction on planetary evolution and the creation of stellar heat by the influx of meteorites.

Liebig accepted Mayer's preliminary paper. He himself had inklings of the conservation of energy, also gained from the consideration of physiological combustion and general phenomena.

Mayer's perception of the principle of the conservation of energy arose from his critical examination of Lavoisier's theory of physiological combustion, and Lavoisier's

quantitative investigation of combustion arose out of the philosophical study of heat measurement inspired by the steam-engine and the demand of industrialism for the measurement of commodities in order that their value could easily be ascertained for the purposes of commerce.

Liebig's appreciation of Mayer's papers was characteristic of his deep intelligence. His equal as a natural philosopher has not been seen among his successors in chemical science.

Mayer's first paper received very little attention apart from Liebig's acceptance for publication. He had become a practising medical doctor in his native town, and his lack of academic position prevented him from forcing his ideas on to the academic world. His townsmen ridiculed his pretensions to intellectual originality and confirmed their opinion by pointing out that his claims had been ignored by the professional scientists. The mental agony caused by this experience drove him mad, and he was confined for a period in a mental hospital. In 1862 Clausius privately supplied J. Tyndall with information about him and copies of his papers. On the basis of this material Tyndall engaged in a controversy with W. Thomson and P. G. Tait, who had not given sufficient recognition to Mayer. He wrote with a personal candour that has rarely been equalled in subsequent scientific controversies.

In 1843 the Danish scientist, Colding, also argued in favour of the equivalence of heat and mechanical effect.

Reynolds comments that philosophical reasoning was not sufficient to remove the prejudice and casuistry respecting caloric. Mayer's brilliant reasoning convinced few. Demonstrations with results of barn-door obviousness were required. Joule produced such results with exquisite experimental perfection and clarity of thought. But it is possible that much of Joule's perfection was a concession to human stupidity. A less prejudiced audience would have required less belabouring with exactitude in order to learn the truth. The demand for experimental proof can be extravagant. It has been remarked as curious that Clerk Maxwell did not search experimentally for the electromagnetic waves whose existence he had deduced theoretically.

Perhaps his intelligence was satisfied with his physical reasoning and he did not feel the need of experimental proof.

Mayer had the misfortune to arrive at a physical discovery from biological considerations. In his first paper he did not give his physiological arguments, and the physical demonstration of his physical result was not in itself full enough to convince a prejudiced physical audience. The physicists wanted to have physical results demonstrated according to their own criteria. They believed the evidence of their own science was better than evidence from other sciences. This prejudice was shown by William Thomson in his controversy with the geologists concerning the age of the earth.

The physicists could be convinced of the principle of the conservation of energy only by comprehensive evidence from their own science, and from the most fashionable part. The discovery of electro-magnetism by Ørsted in 1820 had stimulated an immense interest and activity in electrical researches. In 1821 Faraday had demonstrated the mutual rotation of a current conductor and magnet, and in 1825 Sturgeon invented the soft-iron electro-magnet. With the aid of the electro-magnet Cooke and Wheatstone constructed the first practicable electric telegraph in 1837. Ohm discovered his law of resistance in 1827. Faraday discovered electro-magnetic induction in 1831, and had already demonstrated by electrolysis the rotation between quantity of electricity and chemical equivalents. In 1836 Sturgeon invented the commutator, which allowed a direct electric current to be obtained from a dynamo, and a motor to be driven by a direct current from a Voltaic cell.

Joule's experimental study of these machines produced the evidence which convinced physicists of the truth of the principle of the conservation of energy. When he began his researches in 1838 there was already a large quantity of fragmentary knowledge concerning the electric current and its relations with heat and chemistry. A comprehensive examination of the known facts might have provided evidence for the equivalence of different forms of energy,

but progress was not gained by that method, though the philosophical inspiration of Faraday's researches was his deep conviction of the equivalence and relation between all forms of force. His experiments on the Identity of the Electricities and the Polarization of Light by the Magnetic Field are pure products of this belief.

Joule started from an entirely different point of view. He was not searching for a proof of any philosophic principle. He was inspired by the possibility that the attractive forces of electro-magnets as applied in Sturgeon's electric dynamos and motors might displace steam as the source of power and motion, and began to investigate how these machines might be improved. He approached the new electrical machines in the perspective of the power engineer. If the electric motor was to be of use to industry it must produce the maximum mechanical effect for the minimum consumption of current. As a first improvement in the electric motor Joule sought to improve the design of the electro-magnets and increase the amount of magnetic force for turning the motor. His first short paper, dated January 8th, 1838, was published in Sturgeon's *Annals of Electricity*. He starts his letter with the paragraph:

"Sir,

I am now making an electro-magnetic engine, and as I imagine that I have succeeded in effecting considerable improvement in the construction of the magnets and the whole arrangement of the instrument, I hope you will allow me to lay it before the numerous readers of your valuable 'Annals.'"

Then he gives a description, with exact dimensions and diagrams, of his twenty horse-shoe electro-magnets with their forty poles fixed alternately in a ring. He claims that this arrangement keeps the wire that excites each pole close to the iron, that the coil that excites one pole also helps to excite the neighbouring pole, that a great saving of room is effected, and that the rotating parts are kept close together. "I have made several magnets of the above construction. Their lifting power is very good. The spark on breaking

battery contact is, however, remarkably brilliant, which may be considered in some respects disadvantageous."

The rotor consisted of a similar series of electro-magnets mounted around an axle and arranged to rotate opposite the fixed ring of electro-magnets. The fixed ring was excited by the current from one battery, and the rotating ring by the current from another. The necessary reverses of polarity in the rotating ring were arranged by a commutator.

On December 1st, 1838, while he was still nineteen years of age, he communicated another paper. His first paragraphs are:

"DEAR SIR,

In Vol. II, page 122 of your interesting work is a communication of mine describing the method of making electro-magnetic engines, which I thought might be adopted with advantage. I finished the one I was working at during last summer. It weighs 7½ lb.; and the greatest power I have been able to develop with a battery of forty-eight Wollaston four-inch plates was to raise 15 lb. one foot high per minute, in which estimate the friction of the working parts, which was very considerable, was reckoned as the load.

The result shows that the advantages of a close arrangement of electro-magnets are not such as I anticipated."

Reynolds remarks that this passage contains the first recorded absolute measurement of work in connexion with the philosophical study of physics, and shows Joule's early appreciation of the measurement of "work," though he does not use the word as the expression of the mechanical potency of his machine.

He has continued his attempts to improve the strength of his electro-magnets, and writes that bundles of iron wires are more effective than solid cores. He has designed a convenient motor for comparing the velocities of rotation produced by the two sorts of cores, and finds that while the solid core gives 146 revolutions per minute, the wire core gives 177 revolutions with one cell. Two cells give 233 and 274. Another pair of electro-magnets gave 196 and

192 revolutions with one cell, and 283 and 321 with two cells. Hence a laminated armature generally gave a large increase in revolutions with the correct intensity of current.

In his third paper he describes the results obtained with armatures made of rectangular wires in order to avoid the wastage of space in the packing of circular wires. The rectangular wire armature gave 162 revolutions, whereas the corresponding solid one gave 130. This is followed by more investigations of the construction and properties of magnets. "The resistance which iron opposes to the instantaneous induction of magnetism is of considerable importance." He finds that a steel magnet could attract a small magnet attached to the arm of a balance with a force of 32 grains at a distance of half an inch, and could lift a weight of sixty ounces. One of his electro-magnets exerted an attraction of only 5·1 grains at half an inch, but could lift ninety-two ounces. On the basis of his measurements he gives various rules for the efficient design of electro-magnets.

In his fourth paper, dated May 28th, 1839, he describes a galvanometer which he has calibrated by electrolysis. The current which deflected the needle was sent through a water voltameter and the size of the deflections compared with the volume of mixed gases evolved. With this apparatus he measured the attractive power of numerous electro-magnets excited by currents of various strengths and tabulated his results. From the figures he deduced a new law, that "*The attractive force of two electro-magnets for one another is directly proportional to the square of the electric force to which the iron is exposed; or if E denote the electric current, W the length of wire and M the magnetic attraction,* $M=E^2W^2$."

At the end of the paper he writes that "I can hardly doubt that electro-magnetism will ultimately be substituted for steam to propel machinery. If the power of the engine is in proportion to the attractive force of its magnets, and if this attraction is as the square of the electric force, the economy will be in the direct ratio of the quantity of electricity, and the cost of working the engine may be reduced *ad infinitum*. It is, however, yet to be determined how far

the effects of magnetic electricity may disappoint these expectations."

This passage shows that Joule did not yet see any fundamental objection to perpetual motion, and that his belief in the future of electrical machinery was an important stimulus, perhaps the chief, to his researches.

In his next two papers he gives more tests of his new law and a description of a new design of electric motor which "enables me with ease to place the electro-magnets in different positions, and as their several coils are insulated, and I am therefore enabled to use the electric current in quantity and intensity arrangements, it offers facilities for experiment. In my preliminary trials I have been much pleased with its performances."

Joule was twenty years of age when he wrote this passage. His construction of an adjustable experimental electric motor for the elucidation of principle and the measurement of efficiency is an astonishing achievement. It is to be compared with the construction by engineers of variable compression internal combustion engines in modern research, and presents a maturity of mind extraordinary in a youth of his age. His quantitative methods are of the highest psychological interest, as they are contrary to the normal psychology of youth. Young geniuses are usually concerned with qualitative ideas, which they prove in middle-age by an acquired technique of exact measurement. Very young scientists are nearly always impatient of exact measurement. Joule had the middle-aged passion for measurement from his earliest youth. His early papers show a singular genius for measurement, combined with high manipulative skill, constructive invention and clear thought. His written English is very good, and his confidence in his results is unconsciously sublime. The young Davy also had been confident in his results, but his confidence was part of his romantic spirit. Joule's spirit was classical, so the nature of his confidence was more surprising. The classical style of thought becomes ridiculous unless it is based on correct facts. Joule triumphantly succeeded in using it because his facts were right.

In his seventh paper he gives a series of tables containing observations of the performance of his latest electro-magnetic engine. The "relative quantities of electric current . . . the difference between those quantities . . . the velocity of the revolving electro-magnets in feet per second . . . the work including friction . . . the duty in pounds raised to the height of one foot by the agency of one pound of zinc" . . . are noted.

"In calculating the amount of work, I found that current 12·4 was just sufficient to keep the machine in motion, the friction referred to the distance from the axle of the revolving electro-magnets being equal to ten ounces avoirdupois, the same quantity of current was, whatever the velocity might be, always able to overcome exactly the same amount of friction. I therefore felt justified in using it as a basis on which to calculate the force due to other quantities of current electricity. The duty, in the fifth column, is calculated on the basis of decomposition of water by a given current. I must observe that the friction is estimated as part of the work, and that, whenever the motive force was not sufficient by itself to turn the machine, a weight thrown over the pulley on the axis supplied the requisite assistance."

His use of the words "work" and "duty" in this passage virtually marks the introduction of these industrial engineer's terms into physical science. The moment when one great branch of human activity begins to influence another cannot often be clearly defined, but Joule's words here clearly record the moment at which the industrial engineer's conceptions of "work" and "duty" entered physical science. The notions of "work" and "duty" had evolved during the growth of industrial engineering as measures of the economic value of the activity of engines. They were outside physics. Joule effectively introduced them into physics in his analysis of the running principles of the electric motor.

The figures of the running tests given in this paper show the effects of the electrically induced resistance in the wire owing to the motion of the machine. When driven by a constant battery the force of the engine decreases as the

speed increases. This induction effect sets a fundamental limit to the speed of the electric motor, and prevents it from becoming a perpetual motion machine. The electrical effect of induced resistance was discovered by Faraday, and Reynolds writes that Joule, who had been trying for two years to perfect the electric motor in ignorance of it had learnt of it perhaps in recent reading of Faraday's papers. Joule writes that his figures "show pretty clearly the effects of magnetic electrical resistance. This resistance is the prime obstacle to the perfection of the electro-magnetic engine; and in proportion as it is overcome will the motive force increase. It therefore claims our first attention."

Joule's genius for reaching the quantitative aspect of phenomena is illustrated in his next paragraph. Instead of remaining satisfied with the recognition of the existence of the phenomenon, he immediately gives a quantitative statement of it. He writes:

"On comparing the differences with the velocities and currents in each table the general conclusion is that the magnetic electrical intensity resisting the current is directly proportional to the velocity multiplied by the magnetism or, which is the same thing, by the electricity which induces that magnetism." This implies that the amount of electrical action consumed in overcoming the induced resistance in the machine is proportional to the product of the square of the current and the velocity of rotation, and this is proportional to the mechanical effect. Thus the paper contains data which implies that the electric action, measured by the product of electro-motive force and current, is equivalent to the mechanical effect, and to the chemical effect by which the current had been produced.

In his eighth paper, dated August 21st, 1840, Joule discusses the problem of electrical measurement and the influence of magnetic saturation on the performance of electro-magnetic machines. Concerning measurement, he remarks that "The great difficulty, if not the impossibility, of understanding experiments and comparing them with one another arises in general from incomplete descriptions of apparatus and from the arbitrary and vague numbers

which are used to characterize electric currents. Such a practice might be tolerated in the infancy of the science; but in its present state of advancement greater precision and propriety are imperatively demanded. I have therefore determined for my own part to abandon my old quantity numbers and to express my results on the basis of a *unit* which shall be at once scientific and convenient."

He writes that Faraday's standard of *degrees* of electricity, based on electrolysis, is the only one that has been suggested. "However, as I am not aware that it has been used in the researches of any electrician, not excepting those of Faraday himself, I do not hesitate to advance what I think more appropriate as well as generally advantageous."

He then defines a degree of static electricity as the quantity which is just able to decompose nine grains of water. A degree of current electricity is the same quantity propagated during each hour of time. "Where both time and length of conductor are elements, as in electro-dynamics, *a degree of electric force or of electro-momentum is indicated by the same quantity* (a degree of static electricity) *propagated through the length of one foot in one hour of time.*" He explains his choice of unit as follows: "As 9 is the atomic weight of water, it is obvious how greatly this *degree* would facilitate the calculation of electro-chemical decompositions. I may adduce an illustration from electrotype engraving: here, if a galvanometer graduated according to the proposed scale were included in the circuit, it would only be necessary to multiply the degrees of its indication by 32, the equivalent of copper, and the product by the time in hours during which the work has been carried on, to obtain the weight in grains of the copper which has been precipitated; and there would therefore be no occasion to arrest the process until calculation has shown that the proper quantity of copper was cast."

Reynolds cites these passages as evidence that Joule was the founder of the present system of absolute units in science. While not the inventor of absolute units, Joule's system is of the type which has been adopted for practical use.

These passages, written at the age of twenty-one years,

illustrate the qualities of Joule's genius. They show a powerful intellectual appreciation of the needs of the scientific experimental method, an ingenious choice, an understanding of the needs of the industrial engineer, and a magnificent freshness. But their most remarkable quality is their poise. A meditation on its meaning in the Manchester environment of industrial squalor, independent wealth and absence of homogeneous learned society exposes his brilliance.

On February 16th, 1841, Joule gave the first of his rare public lectures, at the Victoria Gallery in Manchester. He gives his opinion of the future of electrical machinery in the perspective of his researches and a theory of ferro-magnetism based on his investigations of electro-magnets. He ascribes to Jacobi the first explanation of the theoretical limits to the efficiency of the electric motor, and then writes that as Jacobi had not given "precise details concerning the duty of his apparatus" he constructed an experimental machine that could be made to provide precise details.

"With my apparatus every pound of zinc consumed in a Grove's battery produced a mechanical force (friction included) equal to raise a weight of 331,400 lb. to the height of one foot, when the revolving magnets were moving at the velocity of eight feet per second.

Now the duty of the best Cornish steam-engine is about 1,500,000 lb. raised to the height of one foot by the combustion of a lb. of coal, which is nearly five times the extreme duty that I was able to obtain from my electro-magnetic engine by the consumption of a lb. of zinc. This comparison is so very unfavourable that I confess I almost despair of the success of electro-magnetic attractions as an economical source of power, for although my machine is by no means perfect, I do not see how the arrangement of its parts could be improved so far as to make the duty per pound of zinc superior to the duty of the best steam-engines per pound of coal. And even if this were attained, the expense of the zinc and the exciting fluids is so great, when compared with the price of coal, as to prevent the ordinary electro-magnetic engine from being useful for any but very peculiar purposes." In this passage Joule concludes his

first research, the attempt to improve the practical value of the electric motor. After three years' intense application he had proved that the future of the electric motor was much less simple than he had initially believed. Incidentally he had discovered a considerable amount of new physical knowledge concerning magnets and electro-magnetism and had introduced engineering methods of absolute measurement into physics.

His interest now moved from engineering to physics, but he took his new quantitative methods with him.

After describing the result of his study of the efficiency of the electric motor, he continues his lecture with the description of some new experiments in ferro-magnetism. A Manchester friend was of the opinion that a bar of iron expanded when magnetized. As the expansion might occur with great force even if only through a slight distance, it might be used as a source of power. He found an iron wire two feet long was increased in length by one thirty-three thousandth of an inch when magnetized by the current from one cell.

"A good method of observing the above phenomena is to examine one end of an electro-magnet with a microscope while the other end is fixed. The increment is then observed to take place suddenly, as if it had been occasioned by a blow struck at the other extremity. The expansion, though very minute, is indeed so rapid that it may be felt by the touch; and if the electro-magnet be placed perpendicularly on a hard elastic body, such as glass, the ear can readily detect the fact that it makes a slight jump each time that contact is made with the battery.

When one end of the electro-magnet is applied to the ear, a distinct musical note is heard every time that contact with the battery is made or broken—another proof of the suddenness with which the particles of iron are disturbed.

With regard to the application of the new force to the movement of machinery, I have nothing favourable to advance."

The last part of the lecture contains a theory of ferro-magnetism based on Ampère's theory and these phenomena

of magneto-striction. He supposes the atoms of iron are surrounded by "atmospheres of electricity moving in planes at right angles to the axis of the magnet." He explains magnetic saturation by supposing "that the electricity which revolves round each atom has a centrifugal tendency." Its speed of rotation is limited by this force, and hence the degree of magnetization is limited.

He points out that, unfortunately, the magnetization of a bar of such iron atoms ought to produce a *shortening*. Years afterwards it was discovered that a bar under strong tension is shortened by magnetization.

He then considers other possible properties of his model of magnetic iron atoms. He imagines "the space between these compound atoms be supposed to be filled with calorific ether in a state of vibration, or, otherwise, to be occupied with the oscillations of the atoms themselves."

He explains the destruction of the magnetic power in iron by heat as due to the disturbance of the magnetic and electric atmospheres around the atoms by the atomic heat oscillations. This prevents the atoms from orientating themselves in one direction.

The retentive power of steel is explained by supposing that carbon atoms combine with the iron atoms and make the magnetic and electric atmospheres lop-sided, so that there is a permanent orientation of magnetic force.

During 1840 Joule had become more and more interested in the physical aspects of his electro-magnetic engines and less in their engineering aspects. He had measured their duty, their production of work. Now he desired to measure the heat they produced while running. This seems to have been a purely philosophical interest, because he had already concluded that the industrial prospects of the electrical motor were poor. He made a thorough investigation of the heat produced by voltaic electricity and communicated a paper on his results to the Royal Society. An abstract was read on December 17th, 1840, and published in the *Proceedings of the Royal Society*; but the complete paper was not accepted for publication, though it contained an account of the experiments from which he had formulated the law

that the heat generated by an electric current in a conductor is proportional to the product of the resistance and the square of the current. Schuster has suggested that the Royal Society referees could not believe so important a law could have been established by experiments described in only five pages of print.

Presumably in reference to this paper Lyon Playfair wrote: "When Joule first sent an account of his experiments to the Royal Society the paper was referred among others to Sir Charles Wheatstone, who was my intimate personal friend. Wheatstone was an eminently fair man and a good judge, but the discovery did not then commend itself to his mind. For a whole Sunday afternoon we walked on Barnes Common, discussing the experiments and their consequences, if true, to science. But all my arguments were insufficient to convince my friend, and I fear that then the Royal Society did not appreciate and publish the researches."

In 1841 Joule published a much longer paper on the same material in the *Philosophical Magazine*. It is arranged in the style of Faraday's papers, and suggests that the first volume of Faraday's collected researches on electricity, which had recently been published, had had an important influence on him.

Schuster has recorded that he once asked Joule how he felt when he heard the Royal Society would not publish his first paper, except in abstract. He answered:

"I was not surprized, I could imagine those gentlemen in London sitting round a table and saying to each other: 'What good can come out of a town where they dine in the middle of the day?'"

The version of his results published in the *Philosophical Magazine* is divided into two chapters, containing together seventy-five numbered paragraphs. He begins by writing that few facts in science are more interesting than those which establish a connexion between heat and electricity, and he hopes his results are of sufficient interest to justify him in laying them before the Royal Society.

— heating power of a wire is determined by immersing

it uninsulated in water and registering the rise of temperature. No appreciable quantity of current takes the shorter course through the water, for there is no evidence of "the evolution of hydrogen or the oxidation of metal." A little practice has enabled him to measure temperatures with certainty to one-tenth of a Fahrenheit degree by stirring "the liquid gently with a feather, and then suspending the thermometer by the top of its stem so as to cause it to assume a vertical position" and reading with his eye at the level of the top of the mercury.

With this arrangement for measuring the evolution of heat and the galvanometer calibrated electrolytically for the measurement of electricity, he immediately arrives at his law that the heat evolved by a current in a given time is proportional to the resistance of the conductor multiplied by the square of the electric intensity. His first comment is that "the above law is of great importance. It teaches us the right use of those instruments which are intended to measure electric currents by the quantities of heat which they evolve." His first thought is of how the discovery will affect the science of measurement, and is a characteristic illustration of one of the chief aspects of his mentality.

The second chapter of the paper describes the measurement of heat evolved during electrolysis. He will measure the heat produced in the cells of the battery. He carefully allows for the heat capacity of the parts of the apparatus, and where this cannot be calculated from tables of specific heats he determines the requisite data by his own experiments. He finds it "convenient to adopt" a "*standard of resistance.*" "Ten feet of copper wire, 0·024 of an inch thick, were formed into a coil . . . its resistance to conduction was called *unity*. Three experiments were made in order to ascertain its heating power."

After the elimination of every source of heat not essentially electrolytic, he shows "the heat which is generated in a given time in any pair, by true voltaic action, is proportional to the resistance to conduction of that pair, multiplied by the square of the intensity of the current."

The solution of the zinc oxide in the sulphuric acid of the cell does not contribute to the current, so he determines in subsidiary experiments the quantity of heat evolved in this reaction, and deducts it from the gross evolution of heat in the cell. The corrections for losses by radiation and conduction are also made.

The investigation of the heat production in batteries is followed by an investigation of that in electrolytic cells. He distinguishes between the resistance of conduction in such cells and the resistance to electrolysis. He determines the latter quantity first. After allowing for it, he again finds that the heat evolution due to the resistance to conduction obeys the same law.

He concludes that "*if the electrodes of a galvanic pair of given intensity be connected by any simply conducting body the total voltaic heat generated by the entire circuit (provided always that no local action occurs in the pair) will, whatever may be the resistance to conduction, be proportional to the number of atoms (whether of water or of zinc) concerned in generating the current.*"

He gives two more related conclusions, and then writes that his results confirm Berzelius' suggestion "that the light and heat produced by combustion are occasioned by the discharge of electricity between the combustible and the oxygen with which it is in the act of combination; and I am of opinion that the heat arising from this and some other chemical processes is the consequence of resistance to electric conduction." His unpublished experiments on the combustion of zinc turnings in oxygen, and Crawford's explosions of mixtures of oxygen and hydrogen also support the suggestion.

He is aware that Peltier has shown a current may produce *cold*. "I have little doubt, however, that the explanation . . . will be ultimately found in actions of a secondary character."

His next four papers contain additions to his extraordinary discovery of the quantitative relations between the number of atoms, the heat and the electricity concerned in the action of an electrolytic circuit.

The first of these papers was *On the Electric Origin of the Heat of Combustion*, and was read before the Manchester Literary and Philosophical Society on November 2nd, 1841. It was the first paper he had read to the Society. "Dalton was present, and for the first time in his life moved the thanks of the meeting to the author of the paper." Joule was elected a member on January 25th, 1842.

The paper contains an account of a large number of experiments on the heat of combustion and the effects of polarization and absorption in Voltaic cells.

The second paper was on the same subject, and was read to the British Association at their Manchester meeting on June 25th, 1842. He mentions that his experiments have shown "that the heat evolved by the union of two atoms is proportional to the electro-motive force of the current passing between them—in other words, to the intensity of their chemical affinity." He shows that he had doubted the correctness of the figures in his previous paper, owing to heat losses, but had since confirmed them by comparison with Dulong's careful experiments.

Reynolds remarks that this is the first occasion on which Joule shows any doubt concerning the results of an experiment. During the first three years of his researches his confidence was never upset. This illustrates one of the most remarkable aspects of Joule's psychology. He was an example of precocious genius.

He still explained his results in terms of the electrical theory of chemical affinity proposed by Davy and Berzelius, and had not yet conceived that the heat produced by chemical combination was a measure of the mechanical work necessary to separate chemically combined atoms, and was independent of the particular agency which effected the separation.

His next paper was *On the Heat evolved during the Electrolysis of Water*, and was read to the Manchester Society on January 24th, 1843.

It shows the same masterly experimental skill and clarity as his earlier papers, but in addition there are signs of a search for equivalence between the phenomena. He

remarks in one paragraph that "if the resistance to electrolysis which is over and above that due to chemical change were not accounted for elsewhere it would prove the *annihilation* of part of the power of the circuit without corresponding effect. We shall see that this is not the case, but that in the evolution of *heat*, where the excess of resistance takes place, an exact equivalent is restored."

He points out that the voltage necessary to separate water into oxygen and hydrogen gas is equal to 1·35 times the voltage of a Daniel's cell. This figure "will very nearly represent the intensity or electro-motive force required for the separation of the elements of water and the assumption by them of the gaseous state. By these means heat becomes "latent," and a reaction on the intensity of the battery takes place without the evolution of free heat." Taking the voltage of a Daniell's cell as unity, those of a Grove's cell and a Smee's cell are 1·732 and 0·731 respectively. A Grove's cell consists of platinum in nitric acid associated with amalgamated zinc in dilute sulphuric acid and a Smee's cell of platinized platinum and amalgamated zinc in dilute sulphuric acid. Their voltages therefore "represent the respective affinities of the positive metals for oxygen which is not in the gaseous state. For the separation of the hydrogen from the oxygen of the water is simultaneous with the union of hydrogen with the oxygen which may almost be regarded as free at the platinum, and these actions neutralizing each other, it follows that the intensity of the current in the case of either positive metal very nearly represents its affinity for oxygen in a non-gaseous state. But with the pairs on Mr. Smee's arrangement two things tend to counteract the effect of this affinity. One of them is the force required to separate the elements of water, the other is that required to give hydrogen in the gaseous form. Hence we have $1{\cdot}732-0{\cdot}731=1{\cdot}001$ with the zinc. . . . We shall take 1·0 as the electro-motive force . . . necessary to separate the elements of water and give hydrogen the gaseous state."

A consideration of the electrolysis of zinc sulphate

solution shows that a voltage of 2·188 is necessary to separate oxygen gas from zinc.

"But we have seen that the intensity of the union of non-gaseous oxygen with zinc is represented by 1·732. Therefore 2·188 − 1·732 = 0·456, the intensity due to the assumption by oxygen of the gaseous state."

A consideration of the electrolysis of copper sulphate again indicates that "We may, I think, take 0·45 as a near approximation to the intensity due to the gaseous condition of oxygen."

"As we have already stated that the intensity necessary to separate oxygen from hydrogen and give the latter the gaseous state is almost exactly 1, 1·45 is, according to the above calculations, the intensity required to electrolyse water. This is not very widely different from 1·35. . . . It would be curious to ascertain whether the same amount of caloric would be evolved by the mechanical condensation of eight grains of oxygen gas."

As Reynolds remarks, this sentence shows that Joule had noticed that part of the voltage of the electrolysing battery is occupied in performing an operation which may be reversed by mechanical means.

He had perceived, in other words, that some of the electrical energy from the battery was consumed in giving motion to the molecules of gas released by electrolysis. This distinction between non-gaseous oxygen molecules without kinetic energy and gaseous oxygen molecules with kinetic energy proved to be a main clue towards the discovery of the mechanical equivalence of heat.

In his observations at the end of the paper he explains there are three obstacles to the flow of current in an electrolytic cell, resistance to conduction which evolves heat; "*resistance to electrolysis without* the necessity of *chemical change* arising simply from chemical repulsion," which produces a "reaction in the *intensity* of the battery," and wherever it exists produces an evolution of heat "equivalent to the loss of heating power of the battery arising from its diminished intensity; and *resistance to electrolysis accompanied by chemical changes*" which in virtue of its reaction produces

a heat that "is rendered latent and is thus lost to the circuit."

"Hence it is that, however we arrange the voltaic apparatus and whatever cells for electrolysis we include in the circuit, the caloric of the whole circuit is exactly accounted for by the whole of the chemical changes."

"Both the mechanical and heating powers of a current are, per equivalent of electrolysis in any one of the battery-cells, proportional to its intensity . . . therefore the mechanical and heating powers of a current are proportional to each other."

"The magnetic engine enables us to convert mechanical power into heat by means of the electric currents which are induced by it. And I have little doubt that by interposing an electro-magnetic engine in the circuit of a battery a diminution of heat evolved per equivalent of chemical change would be the consequence, and this in proportion to the mechanical power obtained."

In a footnote dated February 18th ,1843, he writes that he is testing this proposition.

He finishes the paper with the expression of his opinion that he has proved the Davy and Berzelius electrical theory of chemical heat "beyond all question."

On February 20th, 1844, he attached a remarkable appendix to this paper, but its contents will be discussed after the results of his next two papers have been described.

Between January and August in 1843 Joule investigated the evolution of heat by an electro-magnetic engine placed in a voltaic circuit, according to the suggestion in his paper on the Heat evolved during the Electrolysis of Water. In his account of this research given to the Chemical section of the British Association at Cork on August 21st, 1843, he remarks that "when we consider heat not as a *substance* but as a *state of vibration*, there appears to be no reason why it should not be induced by an action of a simple mechanical character such, for instance, as is presented in the revolution of a coil of wire before the poles of a permanent magnet." But it is necessary to prove that the evolution of heat in one part of a circuit containing an electric motor is not due

to the absorption of an equal quantity of heat in another. Previous experiments had left "it a matter of doubt whether the heat observed was *generated* or merely *transferred from the coils* in which the magneto-electricity was induced, the coils themselves becoming cold." His own experiments on the electrolysis of water had shown that the heat evolved by a Voltaic battery is proportional to the simultaneous chemical changes, and that "the heat rendered 'latent' . . . is at the expense of the heat which would otherwise have been evolved in a free state in the circuit—facts which, among others, might seem to prove that *arrangement* only, not *generation* of heat, takes place. . . ."

The production of cold by the passage of a current from bismuth to antimony, discovered by Peltier, was also a confusing fact, for it proved that "the heat evolved by thermo-electricity is transferred from the heated solder, no heat being generated. I resolved therefore to endeavour to clear up the uncertainty with respect to magneto-electrical heat."

He procured a glass tube about nine inches long and two inches in diameter and sealed at one end. He wrapped the outside with tin-foil to reduce radiation from inside the tube, slipped wooden rings on the tube, and then bound them with flannel. The air spaces provided heat insulation and the separating rings reduced convection in the air. The magnet was built of six plates of annealed hoop-iron, each eight inches long, one and one-eighth inch broad and one-sixteenth thick. The plates were insulated from each other with oiled paper and bound together with oiled silk and wound with twenty-one yards of copper wire one-eighteenth inch thick.

The magnet was put into the glass tube and the space between it and the walls was filled with water.

"After stirring the water until heat was equally diffused, its temperature was ascertained by a very delicate thermometer, by which I could estimate a change of temperature equal to about one-fiftieth of Fahrenheit's degree."

The tube was then sealed, with the end of the magnet's winding wire coming through the seal. It was swiftly fixed

so that it could be rotated on a transverse axis between the poles of a powerful electro-magnet, and the two wires were attached to a mercury commutator so that a voltaic current could be sent through the coil of the rotating electro-magnet and its strength noted with a galvanometer. The tube and its magnet were rotated for a quarter of an hour at the rate of six hundred revolutions per minute. The tube was then swiftly opened and the temperature of the water measured. In his first experiment he found the temperature of the water increased by 0·03° F. when the coil of the rotating electro-magnet had been connected with its exciting battery, but when the coil was not connected the temperature fell by 0·05° F. In his next experiment the figures respectively were 0·07 and 0·05. Hence the mean gain in temperature was 0·10° F.

He now procured a much more powerful electro-magnet for surrounding the rotating tube. It produced "a magnetic energy in the iron superior to anything I had previously witnessed." With this big magnet he obtained a mean gain of temperature of 1·84° F.

He measured the rise in temperature due to the rotation of the iron core alone, and of the rise due to its coiled winding alone, and demonstrated that "*the heat evolved by a bar of iron revolving between the poles of a magnet is proportional to the square of the inductive force*," and that "*the heat evolved by the coil of the magneto-electrical machine is proportional* (*cæteris paribus*) *to the square of the current.*"

He analyses the pulsating nature of the heat production owing to the variation in the strength of a current from a dynamo machine due to the alternations.

He proceeds to "consider the heat evolved by voltaic currents when they are counteracted or assisted by magnetic induction. For this purpose it was only necessary to introduce a battery into the magneto-electrical circuit: then, by turning the wheel in one direction, I could oppose the voltaic current, or by turning in the other direction I could increase the intensity of the voltaic by the assistance of the magneto-electricity. In the former case the apparatus possessed all the properties of the electro-magnetic engine;

in the latter it presented the reverse, viz. the *expenditure* of mechanical power." He found that when one Daniell's cell was connected to the rotating magnet and the rotations reversed, the current from the cell could be balanced by the current from the rotating magnet at 370 revolutions, as the absence of sparks at the commutator "beautifully illustrated."

He analyses the separate heating effects due to the voltaic and the electro-magnetic currents and deduces that "*we have therefore in magneto-electricity an agent capable by simple mechanical means of destroying or generating heat.* In a subsequent part of this paper I shall make an attempt to connect heat with mechanical power in absolute numerical relations."

"It became an object of great interest to inquire whether a constant relation existed between it and the mechanical power gained or lost. For this purpose it was only necessary to repeat some of the previous experiments and to ascertain at the same time the mechanical force necessary in order to turn the apparatus.

To accomplish the latter purpose I resorted to a very simple device, yet one peculiarly free from error."

This consisted of a pair of strings wound round the axle of the driving-wheel with their ends passed over pulleys and attached to pans holding weights.

With this arrangement, which he afterwards adopted in the famous apparatus for measuring the mechanical equivalent of heat by rotating paddles in water, he was able to convert the power derived from a falling weight into a magneto-electric current and then convert the current into heat, owing to its effect against the resistance of the coil. Through the mediation of electro-magnetic induction mechanical work was converted into heat.

In one series of experiments each pan had to contain weights of 5 lb. 3 oz. in order to rotate the magnet six hundred times per minute when the ends of its coil were connected with the battery, but when the coil was disconnected weights of only 2 lb. 13 oz. were needed to overcome the resistance provided "solely by friction and the

resistance of the air." Thus the differences between these weights, being together equivalent to 4 lb. 12 oz., was producing magneto-electricity by its fall. In Joule's units they produce a current of 0·983. From his previous experiments he calculates that such a current should produce a rise of 1·85° F. in the tube. "But as the resistance of the coil of the revolving electro-magnet was to that of the whole circuit as 1:1·13, the heat evolved by the whole conducting circuit" was 2·09°. "Adding to this 0·33° on account of the heat evolved by the iron of the revolving electro-magnet and 0·04° on account of the sparks at the commutator we have a total of 2·46°."

He had determined the heat production of the sparks at the commutator in a previous experiment.

He calculates that the heat capacity of the glass tube, water and electro-magnet is equal to that of 1·114 lb. of water. Hence 1 lb. of water would have been raised 2·46° × 1·114 = 2·74° F., "and this has been obtained by the power which can raise 4 lb. 12 oz. to the perpendicular height of 517 feet.

One degree of heat per lb. of water is therefore equivalent to a mechanical force capable of raising a weight of 896 lb. to the perpendicular height of one foot."

Other experiments gave 1001, 1040, 910, 1026, 742 and 860. As the mean of thirteen results he obtains the figure 838.

In the reprint of his papers in 1881 he suggested that his neglect of the evolution of heat in the big stationary electro-magnet probably accounted for his result being too high.

From his measurement of the size of the mechanical equivalent of heat he calculated that the combustion of one pound of coal should produce heat equivalent to 9,584,206 foot-pounds of work. The best Cornish steam engines gave about 1,000,000 foot-pounds per pound of coal burnt. In spite of this low efficiency the steam engine would never be superseded by the "electro-magnetic engine, worked by the voltaic batteries at present used" because "the mechanical forces of the chemical affinities which produce the voltaic

currents are per lb. of zinc" consumed in a Daniell's cell equal to 1,106,160 foot-pounds, and one pound of coal is much cheaper than one pound of zinc.

In an appendix dated August, 1843, Joule quotes Rumford's theory that friction generated heat, and writes "I have lately proved experimentally that *heat is evolved by the passage of water through narrow tubes*. My apparatus consisted of a piston perforated by a number of small holes, working in a cylindrical glass jar containing about 7 lb. of water. I thus obtained one degree of heat per lb. of water from a mechanical force capable of raising about 770 lb. to the height of one foot, a result which will be allowed to be very strongly confirmatory of our previous deductions. I shall lose no time in repeating and extending these experiments, being satisfied that the grand agents of nature are, by the Creator's fiat, *indestructible*; and that wherever mechanical force is expended, an exact equivalent of heat is *always* obtained."

He writes that his friend Mr. John Davies has discussed with him the possible evolution of heat in animal veins owing to the friction of the stream of blood. "It is unquestionable that heat is produced by such friction," but the whole heat of the system must ultimately come from the chemical changes occurring in it.

He apprehends that if an animal ascends a mountain, "in proportion to the muscular effort put forth for the purpose, a *diminution* of the heat evolved in the system by a given chemical action would be experienced."

He considers the results of his paper require a modification of his views on the electrical origin of chemical heat. "I now venture to state more explicitly, that it is not precisely the attraction of affinity, but rather the mechanical force expended by the atoms in falling towards one another, which determines the intensity of the current, and consequently the quantity of heat evolved." This simple hypothesis explains "why heat is evolved so freely in the combination of gases, and by which, indeed, we may account "latent heat" as a mechanical power prepared for action as a watch-spring is when wound up."

"The hypothesis is, I confess, sufficiently crude at present; but I conceive that ultimately we shall be able to represent the whole phenomena of chemistry by exact numerical expressions, so as to be enabled to predict the existence and properties of new compounds."

These words conclude the appendix to the paper.

Joule started his journey to Cork in the company of his friend Eaton Hodgkinson on August 15th, and on August 21st he read his paper to the Chemical Section. Joule wrote in 1885 that "the paper did not excite much attention, though Apjohn, the Earl of Rosse who was President of the Association in that year, and Hodgkinson were interested."

Reynolds considers the experiments described in this paper were technically the most difficult that had ever been accomplished by a physicist. They are certainly unsurpassed in the history of science.

The combination of superb experimental skill with clear thought and philosophical depth makes this paper the finest expression of Joule's genius. He was twenty-four years of age, and had been engaged in research for five years. Though he was friendly with Dalton, Scoresby, Davies and others, he had worked in extraordinary intellectual independence. His chief supports were his own genius and his father, who, to his memorable credit, liberally financed his expensive experiments.

During the five years of research in the laboratory in his home he had mastered the contemporary experimental and conceptual knowledge of electricity, magnetism, electromagnetism, electrolysis, heat and thermo-chemistry. In all of these branches of physics he had discovered new quantitative laws, and had discovered connexions between them unrecognized by any other scientist in England. He had published all of his results and they had drawn no comment. Reynolds explains that the silence with which they were received showed not that they were rejected, but that they were not understood. They were too far in advance of contemporary experimental science to be within a range of immediate recognition, and the distinguished physicists of

the day had not sufficient confidence in their opinion to suggest acceptance or rejection. The absence of ignorant rejections was an indication of contemporary intelligence. "The 'angels feared to tread,' and perhaps the most remarkable thing is that in this case there were no fools."

Though Joule did not immediately convince his contemporaries, they liked and respected him, as he was appointed the Secretary of the Manchester Literary and Philosophical Society in 1846, and the Secretary of the Chemical Section of the British Association in 1845.

His researches of 1843 had given him a knowledge of the mechanical equivalence of heat, work, electricity and chemical affinity, and of the conceptions of the dynamical theory of heat. He immediately applied his ideas in a series of brilliant experiments and new conceptions of phenomena. Many of his results have become foundations of subsequent science, but though they are great, they are not historically and philosophically so interesting as the experimental route by which he arrived at his new fundamental ideas. When he had proved these ideas he could obtain remarkable new knowledge with relatively little difficulty, because he was confident of the effects for which he was seeking. In his first researches up to the year 1843 he had no well-founded belief to direct his experiments. He had to consider his measurements and judge them without the guidance of a probable theory. So the genius of Joule is seen most brilliantly in the early papers, where he exhibits clear thought, conceptual profundity and marvellous experimental skill, without positive support from his contemporaries, or from a personal theory that he knows is probably true. He had to judge the probability of the exactness of his experiments without knowing the law of the conservation of energy. He could not reinforce the findings of his hands and eyes by the intellectual suggestions of a law unknown to him. In a letter to Balfour Stewart, Clerk Maxwell wrote of Joule: "There are only a very few men who have stood in a similar position and who have been urged by the love of some truth, which they were confident was to be found though its form was as yet undefined, to

devote themselves to minute observations and patient manual and mental toil in order to bring their thoughts into exact accordance with things as they are." His manipulative skill was unsurpassed, but his confidence was the most remarkable of his gifts. Having seized with inescapable experimental grasp great ideas about the equivalence of natural forces, and their implication that heat is a mode of motion, he swiftly made numerous profoundly illuminating experiments and conceptual essays. He went back to his paper *On the Heat evolved during the Electrolysis of Water*, and added an Appendix dated February 20th, 1844.

He applies the theory of heat as a mode of motion to the explanation of the heat of vapourization of liquids, to the heat of chemical combination and its equivalence with the electrical force necessary for electrolytic decomposition, and other phenomena. Then he conceives heat as the momentum possessed by atmospheres of electricity that whirl round atoms, while temperature is measured by the velocity of the "exterior circumferences."

He says the theory satisfies the phenomena of conduction, and the law of Boyle and Mariotte for elastic fluids. "When applied to the doctrine of specific heat, it demands the extension of the law of Dulong and Petit to all bodies . . . and points out the following general law. . . . *The specific heat of a body is proportional to the number of atoms in combination divided by the atomic weight.* . . . According to this theory, the zero of temperature is only 480° below the freezing-point, indicating that the momentum of the revolving atmospheres of electricity in a pound of water at the freezing-point is equal to a mechanical force able to raise a weight of about 400,000 pounds to the height of one foot."

This passage marks the discovery of the existence and position of the absolute zero of temperature.

Later in the year he communicated another great paper to the Royal Society, which again published an abstract only, *On the Changes of Temperature produced by the Rarefaction and Condensation of Air*.

He quotes the experiments of Dalton which showed that about 50° of heat are evolved when air is compressed to

one-half of its bulk, and that 50° are absorbed by a corresponding rarefaction. "But our knowledge of the specific heat of elastic fluids is of such an uncertain character, that we should not be justified in attempting to deduce from them the absolute quantity of heat evolved or absorbed." He avoids this difficulty by surrounding his apparatus with water whose temperature may be measured.

As the changes of temperature in the large quantity of water required to surround the condensing pump and receiving chamber must be very small, a very delicate thermometer was required. Joule described his own method of constructing a special thermometer with which he could measure temperatures to two-hundreds of a Fahrenheit degree. The water was contained in a double-walled tank, with air between the walls.

After the temperature of the water, which weighed 45 lb. 3 oz., had been noted, dried air was pumped into the receiver until the pressure had attained about twenty-two atmospheres.

The pumping took about twenty minutes. The water was stirred for five minutes, and its temperature was again measured. In his first series of experiments he found the rise of temperature was 0·643°, of which 0·297° were due to the friction of the pump, established by working the pump without admitting air to the compressor.

The heat from the operation was distributed through the water, the metal of the pump and receiving chamber, and the tank, and was equivalent to 13·628° F. for one pound of water. From Boyle's and Mariotte's law he calculated that the compression must have absorbed work equivalent to 11,220·2 foot-pounds. The division of this number by 13·628 gives 823 as the mechanical equivalent of heat. The next series of compressions gave 795.

In order to confirm his opinion "that the heat evolved was simply the manifestation, in another form, of the mechanical power expended in the act of condensation," he placed two almost equal copper receivers, joined by a tube and stop-cock, into a water tank. One receiver was filled with air at about 22 atmospheres, and the other

was exhausted. When the temperature of the water had become uniform and constant, the stop-cock was opened, and the air allowed to rush from the filled into the empty receiver. The tank water was stirred and its temperature again measured. No nett change in temperature could be observed, so Joule deduced that "*no change of temperature occurs when air is allowed to expand in such a manner as not to develop mechanical power*."

He repeated the experiments with the two copper receivers and the connecting tube in three separate water tanks. He found that 2·36° of cold was produced in the water surrounding the receiver which had contained air at pressure, and 2·38° was produced in the previously empty receiver, while 0·31° appeared in the water surrounding the stop-cock tube. After allowance for conduction of heat into the cold receiver, these figures satisfactorily balanced.

One of the copper receivers, containing air at high pressure, had a spiral leaden pipe attached to the stop-cock. The receiver and spiral were put in a water tank, and the air allowed to escape through the pipe into a jar by which its volume could be measured. The cold produced was equal to the quantity required to decrease the temperature of 1 lb. of water by 4·085° F. The work needed to compress the gas was 3,352 foot-pounds, giving 820 as the mechanical equivalent of heat.

Joule writes that these results are inexplicable on the assumption that heat is a substance, but are "such as might have been deduced *a priori* from any theory in which heat is regarded as a state of motion among the constituent particles of bodies."

He concludes the paper by commenting that his results appear to be in contradiction with the views of Carnot and Clapeyron, who suppose no heat is lost in the working of the steam-engine. "The theory here advanced demands that the heat given out in the condenser shall be less than that communicated to the boiler from the furnace, in exact proportion to the equivalent of mechanical power developed."

By these experiments Joule proved to a high accuracy

the assumption previously made by Mayer, that in the compression of air the whole of the work done is converted into heat, and none is absorbed in changing physical forces operating within the molecules or between them. His reference to Carnot is the first published by any of the founders of thermodynamics.

In 1846 Joule published in collaboration with Scoresby a paper on the comparative capabilities of electro-magnetism, steam and horses as sources of motive power. Scoresby had stayed with the Joules during the Manchester meeting of the British Association in 1842, and became "greatly interested in the view I was at that time beginning to take of the relation between heat and other forms of force, and in response to my express wish to work with a powerful arrangement of magnets, he kindly invited me to Bradford, of which town he was at that time the Vicar, in order to pursue an inquiry along with him."

Joule had to do most of the work, as Scoresby's parish work left him little free time. This vicar was "eminent for qualities seldom united in one man. At once an experienced seaman, a successful geographical discoverer, a hard-working and eloquent clergyman, he was also a zealous student of nature and a scientific investigator."

When Joule and his brother first visited Scoresby they travelled by rail as far as Brighthouse (Brighouse), "each riding on the roof of a first-class carriage, as they frequently did." They attended their host's services, and it is interesting to learn that Joule had the courage not to forget his rule of sleeping through sermons, even from this redoubtable and eloquent clergyman.

With the help of Scoresby's powerful electro-magnets Joule obtained larger outputs of work from his electro-magnetic engine. He finds that the best Cornish steam-engines produce only "one tenth of the *vis viva* due to the combustion of coal."

A horse can do 24,000,000 foot-pounds of work in one day. During this period he eats about 12 lb. of hay and 12 lb. of corn. "From our own experiments on the combustion of a mixture of hay and corn in oxygen gas" it

appears that "one quarter of the whole amount of *vis viva* generated by the combustion of food in the animal frame is capable of being applied in producing a useful mechanical effect,—the remaining three-quarters being required in order to keep up the animal heat, and cont."

Early in 1846 Joule competed for a prize offered by the French Academy of Sciences for the best memoir on the heat of chemical combinations. Owing to the failure to comply with the regulations of the competition, his paper was ineligible and remained unpublished for six years. Its contents are substantially the same as those of his papers on the heat evolved by electric currents and its relation to the heat of combustion.

Like the Royal Society, the French Academy of Sciences was not happy in its treatment of Joule's first communication, though in the next year they became the first of the national scientific societies to accept from him a paper on the equivalence of heat and mechanical effect. It contained a determination of the mechanical equivalent by the friction of a paddle wheel revolving in mercury.

Joule's genius and his life matured in 1847, when he was twenty-eight years of age. He gave a lecture in the reading-room of the St. Ann's Church, Manchester, *On Matter, Living Force, and Heat*, which contained a philosophical statement of the law of the conservation of energy. He begins with definitions of matter, and its property of inertia. He explains "that the force expended in setting a body in motion is carried by the body itself, and exists with it and in it, throughout the whole course of its motion." This force he describes as *vis viva*, or living force, and explains that it would be absurd to suppose it could be destroyed, though that was the common opinion of philosophers. If living force were destroyed by friction, for example, the world would have come to a standstill long ago, so it must be transformed into another thing when it disappears. This thing is heat. He has proved by experiment that work can be transformed into heat, and his experiments on the expansion of gases, which are theoretically similar to the expansion of steam in a steam-engine, have shown heat

can be transformed into work. A steam-engine is merely a machine for turning heat into work.

"The earth in its rapid motion round the sun possesses a degree of living force so vast that, if turned into the equivalent of heat, its temperature would be rendered at least 1,000 times greater than that of red-hot iron."

He explains that shooting stars are small planetary stones which have been vapourized by the heat generated by friction against the earth's atmosphere and that the atmosphere prevents life from being killed by meteoric bombardment.

The living force of the trade winds comes from the conversion of heat acquired by the air near the equator.

"We observe in the motion of our limbs a continual conversion of heat into living force, which may be either converted back again into heat or employed in producing an attraction through space, as when a man ascends a mountain."

"We may conceive, then, that the communication of heat to a body consists, in fact, in the communication of impetus, or living force, to its particles."

He then gives an explanation of the phenomena of melting, latent heat and evaporation, according to this dynamical theory of heat.

Joule had now advanced from the demonstration of the equivalence of heat, electricity, chemical affinity and work, to the statement of the law and consequences of the conservation of energy. He had discovered the law as the outcome of a long series of completely conclusive experiments. He had conceived it clearly and powerfully, and applied it with much imagination. Mayer had deduced the law by the logical examination of facts. He had provided little experimental proof, but had conceived it as clearly, and applied it with even more imaginative power.

Joule was aware of the importance of his lecture, and was anxious to secure its prompt publication. He asked the *Manchester Guardian* to print it, but they offered only to publish extracts chosen by themselves. The *Manchester Courier* was persuaded by Joule's brother, who afterwards

became for twenty years their music critic, to publish the complete lecture. Half of it was printed in the issue of May 5th, and the second half on May 12th, 1847.

Owing to its publication in a newspaper this lecture remained generally unknown until 1884.

The idea of conservation was appearing in various directions by 1847. Three years previously W. R. Grove had published his essay on *The Correlation of Forces*. Faraday's guiding principle was his belief in the equivalence of the different forms of force. In 1847, at the age of twenty-six, Helmholtz published his brilliant essay on the *Conservation of Force*. He showed Faraday's lines of force obeyed the principle of the conservation of energy, and deduced the existence of the oscillatory nature of the discharge from a Leyden jar from the same principle. Helmholtz had been influenced by some of Joule's early papers. This fact, and his deep admiration for English physics, probably prevented him from immediately recognizing the full merit of Mayer's papers.

Joule and Mayer made their early and formative researches in complete ignorance of each other's work. The similarities and differences between their respective researches are equally remarkable.

The British Association met at Oxford on June 23rd, 1847. Following his papers at Cork in 1843, and Cambridge in 1845, Joule again described experiments which gave the mechanical equivalent of heat. His apparatus consisted of a paddle rotated in a liquid, in one series of experiments water, and in the other, sperm-oil. His results were 781·5 and 782·1.

As Joule's previous papers had raised little interest, the chairman of his section requested him to confine himself to a short verbal description of his experiments. "This I endeavoured to do, and a discussion not being invited the communication would have passed without comment if a young man had not risen in the section, and by his intelligent observations created a lively interest in the new theory. The young man was William Thomson."

Thomson was then twenty-two years of age, and had

already published twenty-six papers on mathematical physics. His account of the occasion was slightly different. He wrote in 1882:

"I made Joule's acquaintance at the Oxford Meeting, and it quickly ripened into a life-long friendship. I heard his paper read at the sections, and felt strongly impelled to rise and say that it must be wrong, because the true mechanical value of heat given, suppose to warm water, must, for small difference of temperature, be proportional to the square of its quantity. I knew from Carnot's law that this must be true (and it *is* true; only now I call it 'motivity,' in order not to clash with Joule's 'Mechanical Value'). But as I listened on and on, I saw (that though Carnot had vitally important truth not to be abandoned) Joule had certainly a great truth and a great discovery, and a most important measurement to bring forward. So instead of rising with my objection to the meeting, I waited till it was over and said my say to Joule himself at the end of the meeting. This made my first introduction to him. After that I had a long talk over the whole matter at one of the conversaziones of the Association, and we became friends from thence forward. However, he did not tell me he was to be married in a week or so, but about a fortnight later, I was walking down from Chamounix to commence the tour of Mont Blanc, and whom should I meet walking up, but Joule, with a long thermometer in his hand, and a carriage with a lady in it not far off. He told me that he had been married since we parted at Oxford! and he was going to try for elevation of temperature in waterfalls. We trysted to meet a few days later at Martigny, and look at the Cascade de Sallanches, to see if it might answer. We found it too much broken into spray. His young wife, as long as she lived, took complete interest in his scientific work, and both she and he showed me the greatest kindness during my visits to them in Manchester, for our experiments on the thermal effects of fluid in motion, which we commenced a few years later.

Joule's paper at the Oxford meeting made a great sensation. Faraday was there, and was much struck with

it, but did not enter fully into the new views. It was many years after that, before any of the scientific chiefs began to give their adhesion. It was not long after when Stokes told me he was inclined to be a Joulite.

Miller or Graham, or both, were for many years quite incredulous as to Joule's results, because they all depended on fractions of a degree of temperature—sometimes very small fractions. His boldness in making such large conclusions from such very small observational effects, is almost as noteworthy and admirable as his skill in extorting accuracy from them. I remember distinctly at the Royal Society, I think it was Graham or Miller saying simply he did not believe Joule because he had nothing but hundredths of a degree to prove his case by."

Joule's wife was Amelia Grimes, daughter of the Comptroller of Customs at Liverpool. Her life with Joule was short, for she died in 1854. Their son was born in 1850, and their daughter in 1852. Her early death unfortunately increased his sensitive reserve. He left his own home and came with his children to live in his father's house. Joule was soon troubled by another bereavement, as his father died in 1858. Then, in 1864 his younger brother John died.

His finely poised nature suffered other shocks. In 1858 he was travelling in a train from London to Manchester which was derailed near Nuneaton by a cow that had strayed on to the line. Though three persons were killed, Joule was astonished to see "the engine men eating their dinner with as much sang-froid as though nothing had happened, while the passengers were in a state of the utmost terror. An officer in charge of soldiers was so much excited, that he was brandishing his sword in an adjacent field."

Joule was reading a mathematical book at the moment of the accident. His carriage turned over, and he crawled out. The book was covered with pulverized glass.

He became very nervous of railway travelling, and declined to be nominated again as a member of the Council of the Royal Society, as he wished to avoid travelling to London frequently.

In 1848 Joule was elected a Corresponding Member of

the Royal Academy of Sciences at Turin. It is interesting to note that a foreign society had honoured him before he was thirty years of age. The Italian scientific societies of that date were particularly active, especially in electrical researches.

Joule published a short paper in 1848, extending his conclusions on the nature of shooting stars. He calculates that the friction of a meteorite of the size of a six-inch cube entering the earth's atmosphere at a velocity of eighteen miles per second will produce enough heat to raise 6,967,980 lb. of water 1° F.

Most of this heat will be given to the air, but if the stone receives only one-hundredth, it will be melted and vapourized. "The stone may be considered as placed in a blast of intensely heated air." He writes that in the absence of the atmosphere "no ordinary buildings could afford shelter from very small particles striking at the velocity of eighteen miles per second. Even dust flying at such a velocity would kill any animal exposed to it."

At the Swansea Meeting of the British Association and to the Manchester Society in 1848, Joule gave an excellent statement on his views on the nature of heat and the dynamical theory of gases. He writes that he has now accepted Herapath's explanation of the pressure of gases as proportional to the *vis viva* of its particles. Combining his own theory of heat with Herapath's conception, and using ·491° F. as his latest estimate of the absolute zero of temperature, he calculates that the velocity of a particle of hydrogen gas is 6,225·54 feet per second at 60° F. and 30 inches barometric pressure. At 32° F. it will be 6,055 feet per second. These are the first calculations of the mean speed of the particles of a gas, and agree closely with the latest determinations. Joule explains that the mean speeds of particles of water vapour, nitrogen, oxygen and carbon dioxide are inversely proportional to the square root of their specific gravities; so that an oxygen particle, being sixteen times heavier than a hydrogen particle, will have a quarter of its speed in order to produce the same pressure. Hence the mean speed of an oxygen particle at 32° F. at 30 inches pressure will be 1,514 feet per second.

By 1850 Joule had completed his redetermination of the mechanical equivalent of heat, with the greatest exactitude of which he was capable, in accordance, as he explains, with the pledge he had made some years before to the Royal Society. His paper was communicated by Faraday, and marked the official acceptance of his researches by the chief British scientific society. It is usually quoted in the standard textbooks on heat. In his historical remarks, Joule notices the contributions of Locke, Leibniz, Rumford, Davy, Faraday, Séguin, Grove and Mayer to the development of the theory of heat and the conservation of force.* These are followed by full details of very thorough determinations of the mechanical equivalent of heat.

The boredom with which Joule's researches are often approached is partly due to this paper. It is a careful summary and repetition of old work, and contains no original example of Joule's genius. It illustrates his detailed thoroughness and provides no example of his marvellous power of discovering new facts through imaginative conceptions, discliplined by a few paragraphs of logical argument and simple arithmetic.

After the publication of this paper in 1850 Joule was elected a fellow of the Royal Society, at thirty-one years of age. The importance of his work was recognized quickly, though not immediately. Faraday was thirty-two years of age before he was elected a fellow of the Royal Society. It is possible that Joule's social position as a son of a rich brewer assisted his early election.

By 1850 Joule's scientific reputation was made. He continued to live for thirty-nine years, but the results of the scientific researches made during this period, though numerous and important, are not of the same supreme quality as those made in the first twelve years of his studies. This is due to the improbability that he would find another region of research which could provide such important results and simultaneously suit his particular genius so well. It may also be due to his friendship with William

* It has been said that the Russian chemist Lomonossov first proposed the theory of the conservation of energy and the dynamical theory of gases.

Thomson. Joule had too much respect for Thomson's mathematical abilities. If he had not subordinated his own superior genius to the solution of Thomson's requests, it might have led him to other solitary regions, where it might have made discoveries as great as those it had made in the previously unchartered regions of the experimental theory of heat. In his collaboration with Thomson, Joule behaved like a chief experimental assistant, rather than the collaborator of superior genius. The explanation may also be partly due to a decline of his powers owing to physical or psychological ill-health.

Joule's conceptual and experimental contributions had provided the data for the mathematical development of the theory of heat. This was made by Clausius, Thomson and Rankine. As he was not a mathematician he could not share in this extension.

The new theory of thermodynamics had explained how the efficiency of heat-engines depends on the ranges of the working temperatures, and had shown the limitations of the efficiency of the steam-engine owing to the small range of temperature through which it works. In 1851 Joule described a hot-air engine that could work over a range of temperature much wider than that of a steam-engine. The attempts to operate hot-air engines according to his design were not practically successful, but it is interesting to note that the modern highly efficient Diesel engine is a hot-air engine in which the air compressed in the cylinder is superheated by the combustion of an injected charge of oil. In Joule's engine the air was to be compressed into an externally heated receiver, and the superheated air was then to do work by expansion in the piston cylinder.

The development of thermodynamics created a demand for more exact data concerning the thermal properties of gases. Regnault had published his famous tables of the relations between the volume, density, pressure and temperature of air, and of steam, in 1847. His accurate measurements had shown that though air closely obeys Boyle's law, the density increases slightly faster than the pressure. From this datum the new theory of thermo-

dynamics indicated that air should be cooled slightly when allowed to expand through a small hole without doing work. Thomson suggested to Joule in 1852 that he should measure the temperature of air before and after expansion through a small hole. In preliminary experiments Joule showed that the cooling effect existed. He found small-scale experiments would not provide accurate results, so he designed expansion apparatus driven by a three-horse power steam-engine. The Royal Society provided a grant towards the cost of building this expensive apparatus.

As it was bulky, Joule erected the apparatus in the Salford Brewery. In 1854 the Brewery was sold, so he had to remove the apparatus to his house, now at Oak Fields, Whalley Range. Part of it had to be operated in the open air, as there was not room for it in the laboratory. His brother relates that at this period Joule for some months "could not find time to take his meals properly—just ran in and out again. The experiments were so delicate that many were carried out in the night, because a cab or cart passing along the road disturbed them, though the laboratory was at the back of the stables."

The collaboration of Joule and Thomson on the thermal properties of gases lasted for seven years. They elucidated the cooling effect due to the separation of molecules that attract each other slightly. The expansion of air whose molecules have a slight mutual attraction, produces cooling owing to the absorption of heat in the performance of the work of separation against this attraction.

This effect, known as the Thomson-Joule effect, is now the basis of the industry of liquid air production and low temperature refrigeration. Thomson and Joule found that the effect could be most conveniently observed when the air was allowed to expand through a porous plug.

Joule again removed his apparatus in 1861, when he moved to Thorncliffe, Old Trafford. One of his neighbours objected to the disturbance made by the steam-engine, and successfully prevented him from continuing his experiments. Joule was deeply upset by this action. He had recently done careful research on condensation of steam on metal

es. This led to the improvement of the design of the condensers of steam-engines.

This research was ended by the unfortunate affair 1861, and Joule did not do much research during the decade.

Between 1852 and 1861 he made other interesting researches besides those on the Thomson-Joule effect. In 1855 he suggested the now widely-used method of electric welding. Owing to cost the first experiments were made in Thomson's laboratory at Glasgow.

A bundle of iron wires was welded by surrounding them with charcoal and transmitting a powerful current through them. Joule followed this by welding "several steel wires into one, uniting steel with brass, platina with iron, and cont. I doubt not but that in many instances the process would advantageously supersede that of soldering. . . ."

Between 1856 and 1859 he made experiments to confirm his theory of shooting-stars. These arose in his general studies, made in collaboration with Thomson, of the thermal effects of fluids in motion. It is interesting to note that the phenomena of the surface condensation of steam and of shooting-stars are governed by the same principles of fluid motion.

At the Aberdeen Meeting of the British Association in 1859 Joule described the effects of whirling thermometers and thermo-electric junctions through the air. In the first experiments the thermometers were simply whirled round at the end of a string. This was sufficient to prove that rapid whirling produced a higher rise of temperature than slow whirling. The thermometers were more rapidly whirled by attaching them to the spindle of a lathe. In collaboration with Thomson he found that the temperature rose proportional to the square of the velocity, and that the velocity necessary to raise the temperature 1° C. was 182 feet per second.

The thermo-electric junctions were connected with a Thomson galvanometer. The readings showed that the wires were raised 1° C. in temperature when whirled at 175 feet per second.

At 372 feet per second the rise was 5·3° C. "Thus at a mile per second the rise of temperature would be in round numbers 900° C.; and at twenty miles per second, which may be taken as the average velocity with which meteorites strike the earth, 360,000°."

He infers the rise is due to the stoppage of air and is independent of its density, so there is no longer any doubt concerning the nature of shooting-stars.

Among the researches made by Joule under Thomson's influence were interesting investigations of the thermal effects of strains on solids and liquids. Thomson had calculated, according to the new thermodynamics, the degree of heating that should occur in deformed elastic materials. For instance, a piece of india-rubber, when placed between the lips and stretched, produces a sensation of warmth, as Gough had previously observed. Joule measured these effects and found the temperature of a spiral spring rose by 0·00306° F. when compressed, compared with 0·00406° F. according to the theoretical prediction.

In 1871 Joule published his experiments *On the Alleged Action of Cold in rendering Iron and Steel Brittle*. The winter of 1870 had been very cold and there was a large increase in the number of fractures of railway equipment. Many persons suggested the fractures of iron and steel were due to the cooling by the frost. Joule started to disprove this pretence "set up to excuse certain Railway Companies" for not having made adequate allowances for the effect of the hardness of the ground "which is the common-sense explanation of the accidents." He experimented with darning-needles and garden nails, and proved that they were about one per cent. stronger at 12° F. than at 55° F. Many engineers questioned the relevance of experiments made with darning-needles to the behaviour of railway equipment. Joule was excited by the criticism and replied with zest. This was one of the very rare occasions on which he engaged in controversy.

In 1872 he invented a mercury displacement pump. Reynolds states this was the first displacement pump. The development of displacement pumps, of which Sprengel's

form is well known, has had an enormous influence on physics. Without the high vacua obtainable with these pumps the modern investigations of the discharge of electricity through gases would have been impossible, and the electron would probably not yet have been discovered.

He was elected president of the British Association for the meeting at Bradford in 1873, but, as already explained, he was not well enough to deliver the address and resigned.

About the year 1875 he began a long Verification of the Mechanical Equivalent of Heat, for the British Association. He was unable to continue this repetition of his measurements at his own expense, as the value of his investments had declined and he had become poor. The Royal Society accordingly granted him £200 for the expenses of the investigation. The results of this work form the content of his last published paper, which appeared in the Philosophical Transactions of the Royal Society in 1878.

In the same year his poverty was relieved by the grant of a pension of £200 per annum by the Government.

His last researches were made in his house at 12 Wardle Road, Sale, into which he had moved in 1877. He used an outbuilding as his laboratory, and stored his apparatus in the cellars. The magnificent collection of Joule's notebooks and apparatus, possessed by the Manchester College of Technology, was found in the cellars years later by the late Professor Haldane Gee and acquired by him for the College.

Apart from the preparation of an edition of his collected papers and joint papers he did little more scientific work. He became still more reserved, seeing few persons except his family, and after a long illness died in 1889.

Joule's immense genius was of the sort adopted to the solution of one supreme problem. Its perfection made it the perfect instrument for the solution of the one supreme problem suited to its style. It could operate perfectly or not at all. This is the explanation of his devotion to one problem. His genius rejected problems beneath and

unsuited to it. He had a profound sense of the æsthetic of science. The quality of his work resembles that of Leonardo da Vinci. There is a psychological similarity in their pursuit of perfection. Both returned to the same subjects and continued the refinement of technique, subtle thoughts following the solitary contemplation of the results of their accessions of manual skill.

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IV

WILLIAM THOMSON
LORD KELVIN

1824–1907

IV

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WILLIAM THOMSON was one of the great British personalities of the nineteenth century. His greatness is certain, but like Mr. Gladstone's, its exact nature is not obvious to the twentieth century. As with Davy his purely scientific achievements were extraordinary, but not commensurate with his prodigious reputation. His contemporary fame was due to several factors, of which his work in pure science was one only. His contributions to the theory of electricity, to the theory of energy, to electrical engineering, to geophysics, to the mathematics of physics, to meteorology, to navigation were very important, but in no case commanding. In those instances where he produced germinal ideas he did not develop them and he did not endow those branches of science, to which he contributed large bodies of research, with the intellectual attitudes that have proved to be the most fruitful. World opinion to-day almost unanimously considers Maxwell's contribution to science to be superior to Thomson's. Why is the judgment of the twentieth century different from that of the nineteenth?

Thomson had immense intellectual strength, but was deficient in intellectual taste. He lacked social feeling in the world of living scientific ideas. Some of his admirers noticed that he often seemed to be impenetrable to the ideas of others. This insensitivity was not restricted to

the world of ideas. Helmholtz described in a letter to his wife how, when staying with Thomson, "it was all very friendly and unconstrained. Thomson presumed so much on his intimacy with them that he always carried his mathematical notebook about with him, and would begin to calculate in the midst of the company if anything occurred to him, which was treated with a certain awe by the party. How would it be if I accustomed the Berliners to the same proceeding? But the greatest *naïveté* of all was when, on the Friday he had invited the party to the yacht and then as soon as we were under way, and everyone was settled as securely as might be in view of the rolling, he disappeared into the cabin to make calculations, while the company were left to entertain each other so long as they were in the vein; but you may imagine that they were not very lively. I amused myself by strolling up and down the deck, 'in schwankender Anmuth'."

Helmholtz should have been a keen judge of such behaviour, as his wife, whose opinion he probably shared, has written that "I have set myself all my life against a low level of social environment, and kept it away wherever it was not imposed on me. I have held good manners and a mental equipment superior to my own in some aspect, or interesting at the least, to be the first requirement of social intercourse. In this respect one cannot afford to be modest unless one means to drop into mediocrity."

The eminent Scotsman did not possess the philosophic ability of the eminent German's wife for rationalizing his egoism. Thomson's egoism was direct, but Helmholtz's was adorned by a theoretical foundation. Faraday solved the same problem by renouncing social intercourse, the least disagreeable of the three methods by which these great men secured time for their own interests. The behaviour of all of them is evidence of their social limitations. Thomson was debarred from supreme scientific achievement by his lack of feeling for the tendencies of scientific thought. He could climb over immense mountains of scientific difficulties, but when he reached a peak he was unable to guess what lay beyond the enveloping mist. He was conscious only

of what lay immediately around him. He did not possess the highest power of scientific divination. He was unable to see that the electro-magnetic theory of light was implicit in his own researches. He was deeply interested in gyrostats and rotating motion, but he was unable to see that their natural description should be in the notation of vector analysis. While he was interested in the theory of gyrostats he improved the magnetic compass, but he did not see that the future compass would be gyrostatic. His interest in vortices did not prepare him for the orbital-electron theory of the atom. Unlike the greatest scientists he was unable to divine what lay beyond the immediate facts. In the highest regions of scientific research he was indisciplined. This was perhaps due to his natural and habitual lack of contact with the collective stream of scientific thought. The indiscipline penetrated down into his working habits. He used to write papers in pencil, often on odd pieces of paper, and send them in this condition to the printers. His indiscipline was probably due to a natural tendency, and was perhaps increased by certain features in his education. He never went to school. His father was a teacher of mathematics who became the professor at Glasgow University. He educated his sons, James and William, himself, with much care. It is possible that Thomson's individualistic and intensive education increased his natural tendency to mental isolation because it was individualistic, and because it developed his powerful intellect very early. His prejudice against analytical methods other than the Cartesian may have been partly due to over-teaching in his childhood. He matriculated at Glasgow University at the age of ten and he published twelve research papers before he graduated. He was appointed professor of natural philosophy at Glasgow in 1846, when he was twenty-two years old, and in 1847 he published the mathematical paper that suggested to Maxwell how to develop the electro-magnetic theory of light. In the same year he met Joule and was inspired by him to start the most brilliant of his researches, his development of the theory of thermodynamics. At the age of twenty-three Thomson's work was known throughout

Europe. At about the same age Faraday was Davy's "assistant in experiments and writing" and unofficial valet, and had not yet dreamt of making an original investigation. Thomson's early prestige must have removed the possibility of completing his mental discipline. His discursiveness increased. He rarely prepared his lectures. It is said that for thirty years he never read through a book. He always interrupted the explanations of others and offered numerous suggestions. He rarely studied the literature before he attacked problems. His contemporaries were fascinated by the torrent of researches made with unusually little dependence on the work of others. His power of producing researches of value swiftly and without help was profoundly impressive. His contemporaries were too blinded by the intellectual fireworks to see that few of them were of the very highest scientific quality. If Thomson could have concentrated his intellectual apparatus steadily on to a few fundamental problems he might have become what many of his contemporaries believed him to be, the second Newton. His torrent of papers was a dissipation, besides a product of genius.

While Thomson dissipated a considerable part of his huge intellectual energy he accumulated, through his share in seventy engineering patents, a large fortune, which, after his death, was valued at £161,923. The indiscipline that hampered his scientific genius did not extend to his financial affairs. The success of the first Atlantic submarine cables was largely due to Thomson's theoretical and practical engineering ability, and his driving energy as an organizer. He deduced the correct method of transmitting messages and invented suitable instruments for receiving them. His work on the first cable laid in 1857 made him publicly famous. It also stimulated his interest in the units of electrical measurement, as the cables could not be economically constructed without an exact system of units. This led to the formation of the British Association's famous committee on electrical measurements, and later to the foundation of the National Physical Laboratory. Thomson's increasing distraction after 1857 from theoretical to

applied science prevented him from achieving the commanding position in the history of theoretical science, promised by his astonishing brain. Before the age of thirty-three he had published the researches that laid a large part of the foundations of thermodynamics: he had provided for Maxwell the mathematical clue to the electromagnetic theory of light, and for Hertz to the production of radio oscillations. But Thomson never became the master of any of these fields: Clausius and Gibbs, Maxwell and Hertz explored more widely the vision of the nature of material agencies which these investigations could be made to provide.

The difference between the behaviour of Thomson and Faraday in a similar situation is instructive. When Thomson was invited to devote more and more time to applied science, he accepted the invitation; but Faraday declined it. The probable meaning of this difference is that Thomson did not in his deepest feeling discover a final impulsion towards theoretical science; he did not feel impelled by a spiritual necessity to pursue to its end a theoretical inspiration. Faraday felt that necessity. Perhaps as a theoretical scientist he was more fortunate than Thomson in being free from a strong engineering bent. Whereas Faraday's greatness was to be found entirely in science, a large part of Thomson's was to be found in engineering, and consequently, in sociology. The instruments for submarine cabling and for navigation were manufactured in the workshops of a Glasgow optician, James White. Thomson became the leading partner in White's company. His connexion with electrical transmission and navigation enabled him to accomplish an important part of Davy's programme for the improvement of social life by the application of science to social activities. Amid the grime of the Glasgow factories Thomson showed how physics could be incorporated into the social arts of communication and transport. He secured respect for this important sociological achievement by the conventional method of capitalist society; he made a large fortune out of it. He made a fortune in the midst of one of the toughest groups of industrialists, the Glasgow

manufacturers. During the whole period of his industrial activity he was a professor at the University. Thomson's contribution to social progress, by demonstrating to the governing industrial classes the value of applied science, is as important as his contribution to the development of theoretical science. When he was appointed professor at Glasgow in 1846 he founded the first British university physics laboratory, in which students could learn experimental methods. Glasgow University had had a close connexion with the rising industrial classes, for it had given James Watt the opportunity to construct his revolutionary machinery. In 1756 the Glasgow trade combinations prevented him from establishing a workshop, so he was given the post of mechanician to the University. Thomson's lifelong refusal to leave Glasgow was fortunate. This decision showed, perhaps, the profoundest expression of his genius. His refusal to leave Glasgow was the spiritual parallel to Faraday's refusal to make money. Thomson was on three occasions invited to fill the Cavendish chair of experimental Physics at Cambridge. Most fortunately he refused. He had neither the theoretic vision nor the collective feeling necessary for the foundation of a school of scientific research. He refused to accept a chair in Oxford because he could not imagine science becoming a commanding activity in that University. Though he was professor at Glasgow for fifty-three years he did not found a school of research. His department was always a centre of intense activity, but no scientific visions came out of it. The research problems set to his students were frequently connected with his instrument inventions and, in a degree, turned his university research laboratory into an extra department of James White and Company, where the more recondite problems of design might be solved. Thomson's exploitation of Glasgow University benefited other universities more than Glasgow, but in return for the exploitation there was the benefit of the presence of his personality. All of his students had the educative experience of meeting a great personality, even if they could not learn very much from his teaching. He was a bad lecturer. His inability

to regulate his thoughts prevented him from speaking in a sequence that students could follow. He thought aloud, and often spoke in an inaudible monotone. Sometimes Thomson's courses were delivered by deputies. Near the end of a particularly instructive course from K. Miller in 1870, a student circulated a scrap of paper round the benches of the lecture theatre with the inscription: "Behold the knight cometh, when no man can work." While he lived, science and industry met through his person, but only through his person, in Glasgow. He did not succeed in creating a permanent solution of the problem of the relation of university to industrial science. But the example of his personal success helped the establishment of better science teaching in other universities. The establishment of the Cavendish Laboratory at Cambridge in 1874 was in part stimulated by Thomson's achievements at Glasgow. Until the middle of the nineteenth century university science teaching in Britain had not been orientated in directions of interest to the industrialists who had gained the leadership of British society. Before that date university science teaching had been inspired by the mercantilists of an earlier period of British social development. Under their influence astronomy was the branch of physical science with the highest prestige, because safe navigation was dependent on a knowledge of astronomy, and successful sea-trading was dependent on safe navigation. The prestige of physics in the British universities did not surpass the prestige of astronomy until the importance of industrialism surpassed the importance of mercantilism. The manufacture of machinery, of steam engines, and later of electrical machines made an exact knowledge of the properties of matter necessary to social progress. Thomson was fond of the term "properties of matter." His interest reflected, in a degree, the interests of the industrialist's specialist, the engineers. He was the chief instrument by which the scientific studies of the British universities were reformed to meet the needs of a new governing class. This is the chief explanation of his extraordinary fame, and of his social significance. His contemporaries celebrated him,

as they believed, because he was a great scientist, but he dominated them more because he accomplished the objects of their unconscious social desires.

William Thomson was born in Belfast in 1824. His father was the son of an Ulster farmer of Scottish ancestry, and his mother was the daughter of a Glasgow man of considerable means. His parents were both unusually attractive persons. His father had educated himself and graduated at Glasgow, and had become professor of mathematics in the Royal Academical Institution at Belfast. He combined the diligence of a Scot with some of the Irish temperamental variety. His mother had beauty and charm. She bore a rapid succession of children, and died when William was six years old, leaving a large family of young children. Her husband became something of a mother besides a father to his family. He was an exceptionally gifted teacher, and devoted much of his own time to the education of his children. He loved William particularly, because he was a very beautiful child with wonderful quickness of apprehension, but "most of all, I think, on account of his coaxing, fascinating ways, and the caresses he lavished on his 'darling papa,'" according to William's sister, Elizabeth. He used to talk baby language to his father after he had learnt to speak maturely, and his brothers and sisters sometimes suspected him of affectation. He seemed to pet his father more than his father petted him. In the spring the father used to take his children for walks before breakfast. He invented "interesting topics of conversation, which were carried on through successive mornings." They had intensely interesting talks on the progress of civilization, to which "even little Willie" contributed ideas. William, and his brother James, who was two years older, were from their earliest years very carefully taught mathematics by their father. In 1832 the professor was appointed to the chair of mathematics at Glasgow. The College of the University was then situated in the High Street, adjoining shocking slums. It contained the professor's lodgings, besides the lecture rooms. These compact arrangements allowed the cultivation of a lively

social life among the professors, and also put them near the centre of the town's life. From their quiet rooms, as G. G. Ramsay said, the roar and the flare of the Saturday nights could be heard, "with the cries of carouse or incipient murder." In "the exhausted lassitude of Sunday mornings," "poor slipshod creatures might be seen, as soon as the street was clear of churchgoers, sneaking over to the chemist's for a dose of laudanum to ease off the debauch of yesterday." The professorial families had conversations with old women, not twenty feet from their breakfast tables, "who divided the day between smoking short cutty pipes and drinking poisonous black tea." David Wilson writes that in 1884 Thomson was excited by the prospect of the supersession of the steam engine by the electric motor. His pleasure "seemed to be due to a natural horror of the hideousness of our city. My native Glasgow, in the seventies and eighties, was like Manchester, Birmingham, London and many another such place. . . . Each city has its own peculiar abominations. Each seems the worst till you see the next. As for Glasgow, with its dark smoking canopy and dirty rain, its slums and stinking river, it was uglier than any battlefield."

The Glasgow of 1832 could not have yet achieved the smoky horrors of fifty years later. But it was insanitary enough to experience in that year its first epidemic of cholera.

Professor Thomson began to teach his sons Latin and arranged for them informally to attend the University lectures of himself and his colleagues. James and William made models of the Leyden jars and Voltaic batteries they had seen in demonstration lectures. They matriculated at the ages of twelve and ten respectively. Their fellow-students were mainly raw lads from Scottish highland farms, studying theology with the intention of qualifying for the Ministry. Their ages varied from about fourteen to twenty-four years. William won his first prize before he was eleven. Just before he was twelve he received a prize for translating Lucian's *Dialogues of the Gods*. Before he was fifteen he won a prize for natural philosophy, and before

he was sixteen he won a prize for astronomy, and a University medal for an eighty-five page essay *On the Figure of the Earth*. This essay had an important place in Thomson's intellectual life. It contained the germ of his researches on the history of cosmical bodies such as the earth and sun. A. E. H. Love writes that it is a truly astonishing performance for a boy of sixteen. It includes a discussion of the perturbation of the Moon's motion in longitude, and a deduction of the ellipticity from the constant of precession combined with Laplace's hypothetical law of density in the interior of the Earth. Neither of these points was discussed in the standard treatises of the day. William had read Laplace's *Mecanique Celeste* in the Bibliothèque Royale in Paris while preparing his essay.

In 1839 the professor of natural philosophy, Meikleham, was too ill to lecture, and Nichol, the professor of astronomy, deputized. Nichol had an active mind, and followed the progress of science with keen interest. Meikleham and Nichol admired the great French mathematicians, and in 1840 Nichol introduced Thomson to Fourier's *Theorie Analytique de la Chaleur*. In the spring William and the rest of the family were taken by their father on a Continental tour. They were instructed not to take any books with them, so that there should be no distractions from the cultivation of German conversation. Just before leaving Glasgow William noticed that Kelland, the new professor of mathematics at Edinburgh, had criticized Fourier's work, so he concealed the *Theorie Analytique* in his bag. Every day at Frankfort he used to slip down surreptitiously into the cellar to read a bit of Fourier, and he discovered where Kelland was mistaken. He started to write an explanatory paper *On Fourier's Expansions of Functions in Trigonometrical Series*. This was finished after his return to Glasgow, and it was sent to Kelland, who at first received it rather tartly. It was printed over a pseudonym in the *Cambridge Mathematical Journal* in 1841, and was his first published paper.

Thomson had been admitted a student of Peterhouse a few weeks before the paper was published. The identity

of the author soon became known. Thomson had not been in residence more than a few days before he was regarded as a prospective Senior Wrangler. The atmosphere of his student career rapidly became abnormal. His father formed visions of William as the successor of the aged Meikleham in the chair of natural philosophy at Glasgow. The young man was beseeched by his father to behave as a gentleman and yet give no cause for gossip that might reach Glasgow. He was to cultivate the acquaintance of the most distinguished of his fellow-students, to study hard, and yet not neglect his health. As the Cambridge years passed his father became more and more possessed by his own designs, almost to the degree of aberration. The elder Thomson became ambitious beyond the reasonable limits of parental interest, and was saved from nepotism only by his exceptional character. William's own attitude in this atmosphere was very creditable. He seemed far less excited than his relatives whether he would be Senior Wrangler, and obtain the Glasgow chair. He did not bother to train himself to the last detail for the examination. He gave much time to research, and published a dozen papers while his competitors were practising the arts of swift writing, and the other tricks that helped to bring success in Wrangling.

He rowed well enough to win the Colquhoun sculls, and became president of the Musical Society. His expenditure was not small, and was severely questioned by his father. The cost of maintenance at College between October, 1841, and June, 1842 was £230 7s. 8d.; not a small sum in those days. The total for the first three academic years was £774 6s. 7d. His father inquired whether he might have been defrauded, and enjoined the strictest economy. While his family continued to be deeply concerned in his academic progress, he published more research papers. In 1842 his discussion of the linear motion of heat led him to the observation that a given distribution of temperature cannot arise from antecedents going back continuously into the whole of past time. This result, obtained at the age of eighteen, influenced his thought for the rest of his life.

He used to declare that it convinced him that the cosmos must have had a beginning in time, and was not infinitely old. He published another extraordinary paper in the same year, *On the Uniform Motion of Heat in Homogeneous Solid Bodies and its connection with the Mathematical Theory of Electricity*. This paper was written at the age of seventeen and sent to the *Cambridge Mathematical Journal* before he entered into residence as an undergraduate. "The general conclusions established in it show that the laws of distribution of electric or magnetic force in any case whatever must be identical with the laws of distribution of the lines of motion of heat in certain perfectly defined circumstances." These results were obtained through the application of a number of new mathematical theorems which, he learned later, had been anticipated by Green, Chasles, Gauss and Sturm. The analysis in Thomson's paper was incorporated in the text-books as the most compact method of treating the attraction of ellipsoids and ellipsoidal shells; but, as Sir J. Larmor remarks, "more remarkable from a youth at the age of seventeen is the analogy above referred to between electric force and thermal flux, fundamentally illuminating to both and pregnant with the great advances then impending in physical science."

These and other remarkable papers were published over a pseudonym in the *Cambridge Mathematical Journal*. One of his father's friends doubted the propriety of an undergraduate publishing original papers. Thomson did not publish in his own name in the Cambridge journal until he had graduated. While he was preparing more papers for the press he sat for the graduation examination in January, 1845. To the intense astonishment and disappointment of his friends and relatives he gained only the second place. One of the examiners had remarked to a colleague that they were not fit to mend Thomson's pens, but the wonderful youth was beaten on marks by Parkinson, a clever man and a marvellous examinee. In the succeeding Smith's prize examination Thomson easily gained the first place.

Shortly afterwards Thomson went to Paris. He attended the lectures of Regnault, who was then a young man, and

afterwards worked in his laboratory. His father had ascertained that candidates for Meikleham's chair, when it became vacant, would be expected to have a knowledge of experimental manipulation. On the day before he left Cambridge he mentioned during a walk with his tutor Hopkins that he had been searching for an essay by Green, to which he had found a reference in a paper by Murphy. Hopkins said he had at least one copy of Green's essay, so they returned to his rooms. He found that he had three, and presented two to Thomson. After he arrived in Paris Thomson gave one to Liouville. His own achievements and ideas, and the discovery of Green's essay, made a deep impression on Liouville and the French savants. He told Liouville of his method of electric images by which the distribution of electric charges on bodies of various shapes may be elegantly calculated by the application of simple theorems in geometrical reciprocation and inversion. For example, if a point-charge of electricity is placed in front of a plane conducting-surface that is connected to the earth, the resultant electric intensity at points in the space in front of the plane is the same as though the plane's induced charge were replaced by a point-charge in the position of the optical image of the original point-charge. He contributed an article to Liouville's *Journal de Mathématiques* giving the solution of the problem of the mutual influence of two charged spheres, by his method of successive point-images. In an English version of this paper, dated November, 1845, he discusses the mathematical analysis of Faraday's conceptions of the mechanism of electric induction by the polarization of the contiguous particles of the dielectric, and points out the analogy of this process with the mechanism of the conduction of heat, arriving at the notion of "the conducting power of a medium for lines of force" five years before Faraday himself.

Regnault had been engaged by the French Government to measure the constants of heat in order to provide exact information for the development of the steam-engine. He allowed Thomson to start experimental work by holding tubes for him and performing other small tasks. Thomson

renewed his interest in the theory of heat, and read a paper by Clapeyron on Carnot's cycle. He tried to obtain a copy of Carnot's tract but was unsuccessful. He did not find a copy until 1848. After four and a half months in Paris he returned to Cambridge. He was elected a fellow of Peterhouse and attended the meeting of the British Association, which was held in Cambridge in 1845. He became acquainted with Faraday at this meeting, but did not meet Joule, who also attended. In the same year he was appointed editor of what had now become the *Cambridge and Dublin Mathematical Journal*. He energetically sought for contributions and corresponded with the leading mathematicians.

The paper he read to the British Association contained the substance of his paper on the laws of statical electricity, in which he had discussed the mathematical representation of Faraday's lines of force. In some paragraphs that were not reprinted by Thomson in later editions of the paper he suggests a search for an effect in a rotating dielectric analogous to those discovered by Arago in a rotating disc, and also a polarizing effect on a ray of light passing through a glass plate in a state of strain owing to opposite electrical charges in the two faces. The existence of dielectric hysteresis was demonstrated by Röntgen in 1890 and the polarizing effect by Kerr in 1876. After Thomson read the paper Faraday talked with him and sent him a paper by Avogadro for his opinion. In his reply Thomson repeats the chief points of his British Association paper. He writes that "If my ideas are correct, the mathematical definition and condition for determining the curved lines of induction in every possible combination of electrified bodies are very readily expressible." He repeats that he has been unable to find any references to attempts to discover what were afterwards named the dielectric hysteresis and the Kerr effects. Faraday hastened to reply that he had often attempted to discover the effects of an electrically strained dielectric on polarized light, and cites the paragraphs in his *Experimental Researches*, but without success; and that he intends to continue the search as he firmly believes that

the dielectric is in a peculiar state whilst induction is taking place across it. Later, in August, 1845, Thomson wrote again, proposing to call at the Royal Institution to learn more of the experiments, but Faraday was leaving town.

This early contact with Faraday is one of the most dramatic incidents in Thomson's career. It shows at once his tremendous powers and the limitations of Faraday and himself. The youth had foreseen the Röntgen dielectric hysteresis effect and the Kerr effect, he had started the discovery of the mathematical expression of Faraday's conceptions, but through the detachment that was one of his deepest characteristics he had not read Faraday's works thoroughly; though he had the highest regard for Faraday he was unable to establish a communication of intellectual feeling with him. Perhaps Faraday's departure from London was the unfortunate cause. The results of a thorough combination of Faraday's intellectual feeling with Thomson's mathematical power and insight would have heaved human culture. The imperfect combination that was established proved sufficient to fire Maxwell's imagination. This incident also provides one of the most striking proofs of Faraday's inability to collaborate. Thomson was only twenty-one, so the blame for the failure to develop the collaboration must lie chiefly with the elder man. Faraday has said that he had watched for years for a suitable colleague and failed to find one. He ought to have tried Thomson.

In 1845 Thomson published a formula for the mutual induction of two spheres of which one only is insulated. He deduced the force of attraction from the distribution of energy, but the proof was not published until 1849, two years after Helmholtz had published the same method in his *Conservation of Force*. Thomson intended to use his papers as a basis for a treatise on electricity but found that his attempts to work at it systematically proved abortive, "and the prospect of getting it ready is now rather more distant. I have also, since the beginning of the Lent term, been often trying to connect the theory of *propagation* of

electricity and magnetism with the solid transmission of force."

In May, 1846, Meikleham died and his chair became vacant. The decisive action of the long campaign of the elder Thomson and his son had come. The Glasgow electors were submitted to a battery of thirty testimonials, including those of W. R. Hamilton, W. Whewell, A. de Morgan, A. Cayley, G. Boole, H. V. Regnault, J. Liouville and G. Stokes. The other candidates included the professor at Marischal College, Aberdeen; the professor at Fredericton, New Brunswick, and a number of schoolmasters.

On September 11th Thomson was elected. D. King has recorded the expression of the elder Thomson as he came out of the election hall. "A face more expressive of delight was never witnessed. The emotion was so marked and strong that I only fear it may have done him injury."

The Thomsons were meticulously honest in their capture of the chair, but the intensity of their preparations is not very edifying. Their ambition influenced the future of British science in several directions. In order to satisfy the Glasgow requirement of a professor familiar with experimental physics Thomson was continually exhorted by his father to keep his mathematics close to facts of physical interest and to seek every opportunity of acquiring experimental skill. These promptings of ambition certainly helped to overcome the separation between experiment and mathematics in British physical science.

The strenuous operations to secure the appointment to the chair were made with a thoroughness that Thomson rarely showed in his scholarship, though he repeated it later in his business affairs. He was not well read in the literature of his subjects of research and repeatedly rediscovered known facts. He was commendably uninterested in discussions of the priority of his discoveries, but his relatively narrow reading often prevented him from acquiring the correct historical perspective of preceding researches. He was apt to ascribe too much historical importance to the particular researches which had inspired his own work.

On his appointment he began to organize a physics laboratory in which students could do practical work. It was the first of its sort in Britain, and perhaps in the world. G. F. Fitzgerald has described its inception as "the greatest advance that has been made in the methods of education for centuries." The introduction of the method of practical work was no doubt of immense importance, but Thomson himself pointed out that in Glasgow practical work in chemistry was being systematically taught to students by T. Thomson as early as 1818. Thomson was granted £100 for the purchase of instruments and similar sums from time to time. During the first five years of his professorship he developed a simple laboratory in an old wine-cellar adjoining his lecture-room in the old College buildings. Presently he extended his accommodation by quietly annexing a disused examination room. Soon after his appointment he "had occasion to undertake some investigations of electrodynamic qualities of matter to answer questions suggested by the results of mathematical theory, questions which could only be answered by direct experiment. The labour of observing proved too heavy, much of it could scarcely be carried on without two or more persons working together. I therefore invited students to aid in the work. They willingly accepted the invitation and lent me most cheerful and able help. Soon after, other students, hearing that their class-fellows had got experimental work to do, came to me and volunteered to assist in the investigation. I could not give them all work in the particular investigation with which I had commenced—'the electric convection of heat'—for want of means and time and possibilities of arrangement, but I did all in my power to find work for them on allied subjects. It is clear that Thomson did not at once start systematic courses of practical work for students, these grew out of his general employment of students as research assistants.

At this time Lang writes that Thomson "was in the vigour of youth, charming, with a face that shone, a figure lithe and graceful, distinction stamped on the personality."

The old College was surrounded by the most horrible

slums. In 1848–49 Glasgow was visited again by cholera. Thomson's father caught the infection and was not strong enough to recover from it. He died in 1849, having enjoyed the happiness of seeing his son installed beside him at Glasgow and already earning European reputation.

Thomson began his formal duties as professor on November 1st, 1846. On November 28th he noted in his diary: "10.15 p.m.—I have at last succeeded in working out the *mechanico-cinematical* (!) representation of electric, magnetic and galvanic forces. I yesterday evening wrote to Cayley the two first, but I have only this moment got out the last case."

This is the registration of the very moment of the completion of his brilliant paper of four pages, in which he showed that the forces of electricity and magnetism could be represented by the distortions of an elastic solid. He wrote that the clue was to be found in "Mr. Faraday's recent discovery of the affection with reference to polarized light of transparent solids subject to magnetic or electro-magnetic forces." His formulæ for the calculation of the magnetic and electric forces implied that the former involves the differential rotation of the latter. As Sir J. Larmor writes: "If he had probed the matter only a little further he would have been forced to recognize, on Faraday's principles, that it is the time-gradient of the magnetic force that is so related, and the Maxwellian theory of the ether might have opened up to his views. But he winds up the brief and hurried note as follows: "I should exceed my present limits were I to enter into a special examination of the states of a solid body representing various problems in electricity, magnetism and galvanism, which must therefore be reserved for a future paper." The future paper seems never to have arrived, but the present one was enough to give a lead to Maxwell's earliest studies.

On June 11th, 1847, Thomson wrote to Faraday: "My dear Sir, I enclose the paper which I mentioned to you as giving an analogy for the electric and magnetic forces by means of the *strain*, propagated through an elastic solid. What I have written is merely a sketch of the mathematical

WILLIAM THOMSON: THE YOUNG PROFESSOR
Aged 22

(*Kelvin Jubilee Commemoration Volume*, 1899)

analogy. I did not venture even to hint at the possibility of making it the foundation of a physical theory of the propagation of electric and magnetic forces which, if established at all, would express as a necessary result the connection between electrical and magnetic forces, and would show how the purely *statical* phenomena of magnetism may originate either from electricity in motion or from an inert mass such as a magnet. If such a theory could be discovered it would also, when taken in connection with the undulatory theory of light, in all probability explain the effect of magnetism on polarized light."

This communication from the young professor, not yet twenty-three years old, to Faraday astounds and stupefies the imagination. Faraday's inability to give Thomson the right sort of stimulus to proceed, and of Thomson's inability to proceed or to set a colleague or student on the explanation of the implications, is pathetic.

During this period Thomson began an elaborate series of memoirs on the mathematical theory of magnetism. They were far more systematic than any memoirs he had published before or was to publish afterwards. Their aim was to show how the theory of magnetism might be established on facts already known, and how certain assumptions could be purged from the subject. They contained a number of important minor discoveries of method.

While Thomson was hovering around the electromagnetic theory of light, compiling laborious memoirs on magnetism, publishing notes on applied mathematics and organizing new methods of university experimental work, he began the most distinguished series of his researches—on thermodynamics. He was now twenty-three years old.

The history of the discovery of the theory of the conservation of energy and of the mechanical equivalence of heat is complicated. Laplace and Lavoisier in 1780 compared the production of heat by a guinea-pig with the production of heat by burning charcoal in a box surrounded with ice. They found that the amount of ice melted was proportional to the amount of oxygen consumed. This implied, in the

case of the guinea-pig, that its mechanical movements made no contribution to the total output of energy by the animal's consumption of oxygen. The energy from the combustion of oxygen was transformed in the animal partly into heat and partly into mechanical activity. That part which had been transformed into mechanical movement was transformed back into heat by friction against the sides of the vessel and hence helped to melt the ice. Lavoisier used the term "caloric" as almost synonymous with "energy."

In his first experiments at Penzance, when he was a boy, Davy had shown by his experiment of melting ice by friction that heat could not be a fluid, and concluded that it was a motion of the particles of substances. The first statement of the exact convertibility of heat into mechanical effect was published by Mayer in 1842. Thomson was never sufficiently impressed by Mayer's contribution, perhaps because he had learned of the mechanical equivalence of heat from another investigator. The brightness with which Joule's experiments flashed on his mind prevented him from seeing the full merits of Mayer's researches. Owing to his detachment from researches in which he was not personally interested, he wasted a considerable part of his power in repeating independently the discoveries of others. His contemporaries were deeply impressed by these feats, but the historian must to some extent regard them as exhibitions of virtuosity, for they were not discoveries strictly from the point of view of the history of science. In several instances Thomson, by his intellectual power and prestige, was able to secure general recognition for fundamental ideas that had been discovered independently and earlier by others. The deepest ideas in thermodynamics were published first by Carnot, Mayer and Clausius. The ideas of Mayer and Clausius were discovered independently and published soon afterwards by Joule and by Thomson. The acceptance of these ideas was chiefly due to Thomson, probably because he had the weight of British industrial culture behind him. Mayer, the medical doctor of Heilbronn, and Clausius, the young Berlin lecturer, lived in a country that was then at a much lower stage of industrial development. The social

conditions of their country prevented them from receiving the understanding and support that they deserved for discoveries fundamental to the development of a scientific industrialism.

Joule had described some of his researches on the mechanical equivalent of heat to the British Association meetings in 1843 and 1845, without arousing any interest or conviction. He spoke again at Oxford in 1847. Thomson was a member of the audience and had previously decided to rise and criticize Joule's discourse, but as he listened he became convinced that though Joule was wrong in some points, on others he was communicating very important truths; so he decided not to speak in the discussion but to talk to Joule afterwards. He introduced himself and they swiftly became lifelong friends. It is interesting to note that Thomson met two supreme experimenters in his youth, Faraday and Joule, and that he succeeded in establishing a close collaboration with one but not with the other. Perhaps this was partly due to age, as he was only six years younger than Joule, but thirty-three years younger than Faraday. It may have been partly due also to social outlook. Faraday, like Davy, carried into the nineteenth century some of the modes of thought of the educated classes of the eighteenth century. The educated classes of the two centuries were different. Faraday and Davy had been much affected by the culture of the governing class of the eighteenth century. This was the culture of territorial magnates. Thomson and Joule grew up in the Glasgow and Manchester districts respectively, chief centres of industrial development. They had matured in the culture of leaders of industrialism. Perhaps this helped Thomson to know Joule more easily than Faraday.

The influence of industrialism on science is shown by the interesting circumstance that Thomson wrote a short paper on the theory of Stirling's hot-air engine, according to Carnot's principles, two months before he met Joule. At the end of this paper he had suggested that water at 32° F. may be converted into ice at 32° F. without the expenditure of work.

The sociological explanation of the discovery of the theory of the conservation of energy at the beginning of the nineteenth century is connected with the economic notion of exchange value. The economic structure of capitalism could not be operated without an exact knowledge of the equivalence of different forms of energy. An exact price on electrical power, gas, coal and labour power cannot be fixed without an exact knowledge of their relative values, and these cannot be determined without their expression in terms of an entity of which they are various forms. This entity is energy. Hence the capitalistic desire to offer all things for sale forced the discovery of a method of fixing the price of that fundamentally important commodity—power. This led, unconsciously, to the discovery of the theory of energy, the entity of which all powers are exchangeable, and hence barterable, forms. The sociological importance of discussions of priority in scientific discovery lies in their revelation of the rate and conditions of social progress in different communities. Many scientists are punctilious about priority because their careers and incomes are often directly dependent on it. The intimate connection between priority and career has led many scientists who deplore careerism to deplore arguments about priority. Discussions of priority are avoided by many scientists because they often appear to be exhibitions of egotism. But apart from any personal interests, priority has a deep sociological interest. An interesting social discussion on the history of the doctrine of the conservation of energy in France, Britain, Germany and America could be made by references to dates of discoveries without mentioning the name of any scientist. In 1878, a quarter of a century after the rather bitter controversies concerning the priorities of Mayer, Joule, Thomson and Clausius in the discoveries of the laws of thermodynamics, the unpublished papers of the brilliant Carnot, who had died in 1832 at the age of thirty-six of cholera, showed that he had discovered a figure for the mechanical equivalent of heat, and had arrived at correct notions of the laws of thermodynamics many years before Mayer and the others.

Thomson had read Clapeyron's exposition of Carnot's theory of heat engines, and had tried to find a copy of Carnot's essay in Paris. He was profoundly impressed by Carnot's brilliant reasoning, and the more because he had thought about it in Paris during his youth. The intellectual illuminations of his youthful thought by Fourier and Carnot remained very bright throughout his long life. The light he received from Carnot delayed his reception of illumination from Joule and prevented him from fully appreciating Mayer. Apart from personal peculiar habits of thought, Thomson was lacking in knowledge of the history of science and sensitiveness to the sequence of the discovery of ideas. In his youth this was not surprising, for his prodigious intellectual activity before the age of thirty-five must have left him little time for reading. His intellectual behaviour in his later years shows, however, that his lack of historical perspective was more probably due to the lack of a historical sense than of mere distraction from the literature by the press of original thoughts. Comparing Maxwell and Thomson, Larmor writes: "Maxwell's genius was as systematic as Thomson's was desultory."

In 1848 Thomson published his theory of an absolute thermometrical scale, before he fully believed in the possibility of the conversion of heat into work. It was based on the correct part of Carnot's investigation of the theory of heat-engines. Carnot wished to discover the maximum efficiency of a Watt steam-engine by calculating the maximum quantity of power that could be obtained from the production of a defined quantity of heat in the boiler. By analogy with the water-wheel, in which the moving substance falls from a high level through the wheel to a low level, Carnot assumed that heat must fall through the heat-engine from a high to a low temperature, operating the machine during its passage. He assumed that the heat was an indestructible fluid like water, and that it passed through the heat-engine unchanged except in temperature, as the water passes through the water-wheel unchanged, except in height. He supposed that the water, which passed through the engine in the form of steam, merely acted as the means

of transport of fluid heat from the boiler at high temperature to the condenser at low temperature.

Together with this partly erroneous analogy, he had noted that the transmission of heat from a hot to a cold body through the medium of an engine led to the production of mechanical power, whereas the flow of heat from the hot to the cold part of a conductor produced no work. Hence he concluded that the mechanical power of the engine must have been derived from the changes in volume or state produced by the heat in the water. The continued production of power from the engine depends on the repeated addition and subtraction of heat from the water because these operations produced the changes in volume that led to the production of power. Hence continued power was produced through a cycle of operations. He then pointed out that the first condition of efficiency in a heat-engine depended on the degree of its reversibility. The degree of reversibility is merely a measure of the losses, such as those of heat and steam, during the cycle of operations. If heat is lost by conduction through the sides of the cylinder, or steam is lost through leaks round the piston-rod, the engine will not be completely workable backwards. Suppose that no heat or steam is lost. Then if the engine is forced round backwards water-vapour will be pumped from the condenser back into the boiler. The temperature of the vapour in the condenser will fall, and the temperature of vapour in the boiler will ultimately rise, to their original values. If there have been incidental losses through conduction, friction and leakage this restoration cannot be completed and the engine is not completely reversible. All practical engines have such leakages and are therefore not completely reversible. Reversibility is, therefore, the first condition of efficiency in a heat-engine. The second condition is given by the difference in temperature between the boiler and the condenser. The larger the difference the higher the efficiency.

From a consideration of the impossibility of perpetual motion, Carnot deduced that all completely reversible engines operating over the same range of temperature were

equally efficient. Carnot remarked that his assumption that the amount of heat communicated to the vapour in the boiler was the same as the amount of heat remaining in the vapour as it passed into the condenser was not entirely satisfactory. Afterwards he discovered the error in this assumption and calculated the mechanical equivalent of heat, but these discoveries remained unpublished until 1878.

In spite of Thomson's interest and sympathy for Joule, after meeting him in 1847, three years passed before he became convinced that heat was not indestructible and could be converted into work. He was too deeply fascinated by Carnot's exquisite arguments to be able to distinguish the correct from the erroneous part of his assumptions. Still wholly accepting Carnot's theory, he nevertheless succeeded in 1848 in defining an absolute scale of temperature, for he saw that the amount of work done by a perfect heat-engine was dependent only on "*quantities of heat* and *intervals of temperature*," and was independent of the nature of the water-vapour or other substance used in the engine. Hence a scale of intervals of temperature could be calculated without any reference to the expansion or contraction of any particular specimen of mercury or air. This argument was based on the correct part of Carnot's argument, so it did not require logical correction after he had understood that Carnot's assumption of the indestructibility of heat was incorrect.

Thomson made another remarkable deduction from Carnot's theory. He deduced that water at the freezing point may be converted into ice by a process solely mechanical, and yet without the final expenditure of any mechanical work. From the consideration that water expands on freezing, his brother James deduced further that the freezing point of water should be reduced by the application of pressure. He found by experiment that a pressure of 8·1 atmospheres depressed the freezing temperature by 0·106°F. Calculation according to Carnot's theory gave a depression of 0·109° F. This successful prediction of the existence and magnitude of a phenomenon hitherto unknown increased

Thomson's belief in an essential truth in Carnot's theory. At the same time he was equally fascinated by Joule's apparently unanswerable experimental demonstration of the conversion of heat into work. Joule knew his results were incompatible with Carnot's assumption of the indestructibility of heat, and therefore rejected the whole of Carnot's theory. Thomson could not see how the theory of Carnot and the experiments of Joule could be reconciled. Helmholtz was in the same difficulty. On the basis of Joule's experiments and many natural facts, and at the expense of the rejection of Carnot's theory, he had composed his great work on the *Conservation of Force*, which was published in 1847 but not read by Thomson until 1852.

The resolution of the apparent conflict between Carnot's theory and the theory of the conservation of energy was published by Clausius in 1850. This great discovery made him the chief founder of the science of thermodynamics. In his paper Clausius explains how Thomson has clarified and developed the interpretation of the researches of Carnot and Joule and brought out the apparent contradictions between them. But he believes "nevertheless that we ought not to suffer ourselves to be daunted by these difficulties, but that, on the contrary, we must look steadfastly into this theory which calls heat a motion, as in this way alone can we arrive at the means of establishing it or refuting it. Besides this, I do not imagine that the difficulties are so great as Thomson considers them to be, for although a certain alteration in our way of regarding the subject is necessary, still I find that this is in no case contradicted by *proved facts*. It is not even requisite to cast the theory of Carnot overboard, a thing difficult to be resolved upon inasmuch as experience to a certain extent has shown a surprizing coincidence therewith. On a nearer view of the case, we find that the new theory is opposed not to the real fundamental principle of Carnot, but to the addition 'no heat is lost,' for it is quite possible that in the production of work both may take place at the same time; a certain portion of heat may be consumed and a further portion transmitted from a warm body to a cold

one, and both portions may stand in a certain definite relation to the quantity of work produced. This will be made plainer as we proceed, and it will be moreover shown that the inferences to be drawn from both assumptions may not only exist together, but that they mutually support each other."

Clausius showed that Carnot's theory could be deduced from two laws. The first law is the theoretical statement of the mechanical equivalence of heat; that the amount of work done by a heat engine is equivalent to the consumption of heat and vice versa. This law is a particular case of the principle of the conservation of energy. According to the second law, it is impossible for a self-acting machine, unaided by any external agency, to convey heat from one body to another at a higher temperature.

The statement of the second law of thermodynamics requires careful definition before it is completely exact. For instance, Clausius' statement "that heat never passes of itself from a colder to a warmer body" is not correct unless the word "heat" in this context refers to the nett result of an exchange of heat between the two bodies. When two hot bodies are in proximity some heat passes from the cooler to the hotter body, but less than the amount that passes from the hotter to the cooler, in the same interval of time. Unless the word is applied to the nett result of the exchange, Clausius' statement is in conflict with Prévost's law of the exchange of radiation. Thomson's awareness of this sort of logical difficulty delayed his arrival at the second law of thermodynamics, and yet his own statement of the law is erroneous. He writes: "It is impossible, by means of inanimate material agency, to derive mechanical effect upon any portion of matter by cooling it below the temperature of the coldest of surrounding objects." As Maxwell has said, this is not true without further restriction, "for by allowing air to expand we may derive mechanical effect from it by cooling it below the temperature of the coldest surrounding objects."

Thomson did not become aware of the contents of Clausius' paper of 1850 until 1851. Earlier in that year,

before he had heard of Clausius' results, he had independently discovered the method of reconciling the theories of the conservation of energy and Carnot's cycle. In his paper of 1851, where he is describing the circumstances of Clausius' priority and his own later but independent discovery, he does not say whether he was immediately convinced of the force of the reasoning he had independently discovered. Thomson's admirers tended to assume that Clausius had, by fortunate accidents, forestalled him on one or two points in his grand and almost single-handed development of the complete foundation of the science of thermodynamics. Now it is probably true that Thomson could have developed nearly the whole of this science unaided, but in fact Clausius discovered the most fundamental point first, and years afterwards, when the centre of Thomson's interests had moved into other subjects, Clausius continued to improve the mathematical theory of thermodynamics and passed on to fundamental work on the kinetic theory of gases. Clausius and Rankine had assumed in their theoretical investigations that the scale of absolute temperature was virtually the same as that given by the variation of the volume of the permanent gases through changes in temperature. Thomson and Joule began an investigation of the behaviour of gases in order to discover how far it departed from the theoretical conception of the behaviour of a perfect gas. This led to the discovery of the Thomson–Joule effect by which an expanding gas cools itself owing to the extension in average distance between its molecules and the consequent absorption of energy in overcoming the slight attractive forces that exist between molecules of most permanent gases. This cooling effect is very slight relative to the cooling effect due to the conversion of a gas's heat into mechanical work when it is expanded in an engine through a large range of temperature. Hence, as is well known, the Thomson–Joule effect has become the basis of modern low-temperature engineering. The preparation of liquid oxygen and the separation of gases by low-temperature distillation have now become immensely important industrial and military processes. The high

efficiency, and hence suitability, of reversible engines as refrigerators was explained by Thomson in 1852. His brother James afterwards used an engine working on a reversed Carnot cycle in the ventilation of Belfast College. After the fundamental ideas of the theory of thermodynamics had been correctly conceived a magnificent prospect of applications to all of those natural processes involving the transformation of heat became visible. Thomson immediately applied the theory to the elucidation of the behaviour of Voltaic batteries and electric currents, in which heat is produced by the passage of currents. In a short paper of three pages he discussed the *Universal Tendency in Nature to the Dissipation of Mechanical Energy.* He explained that when heat is created by irreversible processes such as friction or conduction, there is a dissipation of mechanical energy and perfect restoration is impossible. He concluded that "any *restoration* of mechanical energy, without more than an equivalent of dissipation, is impossible in inanimate material processes, and is probably never effected by means of organized matter, either endowed with vegetable life or subjected to the will of an animated creature. . . . Within a finite period of past time the earth must have been, and within a finite period of time to come the earth must again be unfit for the habitation of man as at present constituted, unless operations have been or are to be performed which are impossible under the laws to which the known operations going on at present in the material world are subject."

These were magnificent words from the professor, now nearly twenty-eight years old. With them the theory of the evolution of inorganic matter, and of the material universe, passed from the stage of speculation to that of quantitative investigation. Thomson had a major part in the creation of those theories of the evolution of the stellar universe which, in the hands of Jeans, Einstein, Eddington, Lemaître and other contemporary masters, have provided so much stimulation to the human imagination.

In 1854 he showed that if the sun's heat were replenished by the absorption of meteors, according to the suggestion

of Mayer, the total mass of the meteors falling into the sun annually must be about one-fifteen millionth of the sun's mass. Owing to the sun's immense size this quantity of meteoric material would also be relatively large. The presence of so much dust near the sun should influence the revolution of the nearest planets, such as Mercury. Thomson afterwards quoted Le Verrier's discovery that Mercury experiences an unexplained perturbation which might be due to meteoric matter circulating near the sun. He showed that the amount of meteoric matter postulated by Le Verrier to produce this effect was far too little to be sufficient to replenish the sun's heat. It is interesting to note that the explanation of this perturbation of Mercury was first given by Einstein and is one of the classical proofs of the theory of relativity.

In 1854 Helmholtz published his theory of heating by gravitational condensation. According to this theory the source of the sun's heat might be the contraction of the sun's material. As the particles of the sun's matter become packed closer together work must be done by the mutual force of gravitation which pulls them together. This work will be transformed into heat, so there will be a release of heat within the sun. On this principle the sun must contract about one-thousandth of its diameter in order to provide heat for twenty thousand years at the present rate of dissipation. Thomson showed that a combination of this result with a consideration of the specific heat of the sun's material indicated that the temperature of the sun must fall by 100° C. in a time not less than seven hundred, or greater than seven hundred thousand years. He concluded that the sun has illuminated the earth probably for less than one hundred, and certainly for less than five hundred million years. What then was to be thought about geological estimates which allowed three hundred million years for an action such as the denudation of the Weald? In 1865 he published a short paper entitled *The Doctrine of Uniformity in Geology Briefly Refuted*. He showed that if the earth had been losing heat at its present rate for the last twenty thousand million years the total amount of heat lost would

have been sufficient to melt a mass of surface rock equal in bulk to the whole earth. "No hypothesis as to chemical action, internal fluidity, effects of pressure at great depth, or possible character of substances in the interior of the earth, possessing the smallest vestige of probability, can justify the supposition that the earth's upper crust has remained nearly as it is, while from the whole, or from any part of the earth so great a quantity of heat has been lost."

These attacks on the geological theories of gradual change and evolution through vast periods of time, combined with the assertion that the earth had a beginning and an end, and a relatively short life, were immensely popular with the opponents of Darwin's theory of evolution. Thomson seemed to them to be reinstating the Biblical story of the creation in modern terminology. That was probably his own view, for he was a conventional churchman. He conducted his controversy with the geologists more sharply than any other theoretical discussion in which he engaged. This seems to show that he was expressing deep personal feelings besides explaining scientific arguments.

Though the geologists and biologists could not see any way of refuting Thomson's mathematics they remained unconvinced. The evidence of the slow rate of denudation by weathering and erosion, and of the deposition of strata and of biological evolution during the thousands of years of historical record, though of a different sort, was just as good as the evidence of the cooling calculations. In this controversy, as on a number of other occasions, Thomson showed a lack of intellectual sensitiveness or presentiment. He could not feel that evidence from other branches of investigation could be as good as evidence from his own branches of investigation. Because the geologists and biologists disagreed with the physicists he assumed that they must be wrong. They were in fact right. When Strutt had estimated the distribution of radioactive matter in the material of the earth he found that the temperature of the earth might indeed be rising steadily. The twentieth-century discoveries of the energy-content of matter have

removed the necessity for assuming that the sun is cooling rapidly. Its store of heat may be replenished steadily for thousands of millions of years by the annihilation and conversion into heat of its material. With reference to Thomson's partly unsuccessful essay into cosmogony, it is interesting to note that the modern exponents of the expanding universe are tending to repeat his difference with other scientists over the age of the earth. The theory seems to show that if the universe has always been expanding at its present rate it cannot be more than a few thousand million years old. This seems to be far too short a period for the evolution of nebulæ into stars and other cosmical evolutions. But the example of Thomson makes the modern cosmogonists more cautious in concluding that their deductions of a surprisingly small age for the universe are correct.

In the early fifties Thomson's work was not restricted to the independent foundation of the science of thermodynamics. His great paper *On Transient Electric Currents* was published in 1853. The history of this paper provides a dramatic illustration of the scope and limitations or Thomson's genius. He determines with superb clarity and power "the motion of electricity at any instant after an electrified conductor of given capacity is put in connexion with the earth by means of a wire or other linear conductor of given form and given resisting power." He shows that if the capacity of the earth is negligible compared with the capacity of the conductor, and the conductor is without sensible resistance, the problem depends on one variable which can be calculated from a consideration of the energy of the system. The equation for the variable is precisely the same as that "applicable to the circumstances of a pendulum drawn through a small angle from the vertical and let go in a viscous fluid, which exercises a resistance simply proportional to the velocity of the body moving through it. The interpretation of the solution indicates two kinds ot discharge presenting very remarkable distinguishing characteristics: a continued discharge and an oscillatory discharge, one or other of which will take place in any particular case. In the continued discharge the quantity of electricity

on the principal conductor diminishes continuously and the discharging current first increases to a maximum and then diminishes continuously until after an infinite time equilibrium is established. In the oscillatory discharge the principal conductor first loses its charge, becomes charged with a less amount of the contrary kind of electricity, becomes again discharged and again charged with a still smaller amount of electricity but of the same kind as the initial charge, and so on for an infinite number of times until equilibrium is established. . . . If the principal conductor and the length and substance of the discharger be given the discharge will be continued or oscillatory according as the electro-dynamic capacity of the latter, depending as it does on the form into which it is bent, falls short of, or exceeds a certain limit."

He suggests that the apparent oscillatory character of certain types of lightning flashes may be explained in this way. "A corresponding phenomenon might probably be produced artificially on a small scale by discharging a Leyden phial or other conductor across a very small space of air. . . . Should it be impossible, on account of the too great rapidity of the successive flashes, for the unaided eye to distinguish them, Wheatstone's method of a revolving mirror might be employed." To Thomson's great satisfaction Feddersen successfully photographed an oscillatory discharge by this method in 1859. Continuing the account of the original paper, Thomson suggests that this oscillatory discharge phenomenon probably explains Riess' observation of the alternately opposed magnetization of fine steel needles when exposed to certain types of electrical discharge. The observations of Wollaston and Faraday on the anomalous production of mixtures of oxygen and hydrogen gases when water is decomposed by an electrical machine seem also to be explicable by the probable oscillatory nature of the discharge from such machines. Characteristically, Thomson adds a footnote saying that he had discovered the oscillatory nature of certain electrical discharges before he had noticed Helmholtz's conjecture published in 1847, that the observations of Reiss and

Wollaston might be explicable if produced by oscillatory discharges.

This paper of Thomson's is the chief starting point in the experimental discovery of radio waves. In 1854, Clerk Maxwell, who had just graduated at Cambridge, wrote to Thomson for advice on the best method of studying electricity. In the following year he wrote that he suspected Thomson must have important unpublished results, and said: "I do not know the Game laws and Patent laws in science. Perhaps the Association may do something to fix them, but I certainly intend to poach among your electrical images; and as for hints you have dropped about the 'higher electricity,' I intend to take them."

Maxwell told his father that Thomson was "very glad that I should poach on his electrical preserves."

His own genius, and Thomson's admirable spirit of initial encouragement, enabled Maxwell later to discover the mathematical demonstration of the probable existence of electro-magnetic, or radio, waves. But Thomson was not convinced by Maxwell's arguments, and was still sceptical of the existence of electro-magnetic waves when Maxwell died in 1879. By a remarkable irony, the truth of Maxwell's deductions was proved experimentally by Hertz, in 1887, with the assistance of electric oscillators designed according to the principles of Thomson's paper of 1853. The brilliance of this paper, and the failure to divine the deepest of the truths hidden in it, provide the most remarkable expression of the features of Thomson's genius. In 1855 he met Helmholtz for the first time. He had reached, unconsciously, a critical point in his scientific life, for he was about to be drawn into the scientific and engineering problems of the construction of the first Atlantic cables. He was thirty-one years old, and had reached the full height of the purely scientific part of his genius. Helmholtz wrote some weeks after their meeting that he was astonished to find that Thomson was younger than himself, "a very juvenile and exceedingly fair youth," who "far exceeds all the great men of science with whom I have made personal acquaintance, in intelligence, and

lucidity, so that I felt quite wooden beside him sometimes."

Before the end of 1855 he had published ninety-six papers, which included the independent discovery and formulation of the science of thermodynamics, and the clues to the theory and the experimental demonstration of the electro-magnetic theory of light.

He had still fifty-two more years to live.

His interest in submarine cables was stimulated in 1854 by Latimer Clark's investigation of the retardation of signals, the occurrence of which had been predicted by Faraday, as a consequence of the existence of the phenomenon of specific inductive capacity. A cable consists of two conductors, a wire and the sea, separated by an insulator, gutta-percha. Electrically, it behaves like a condenser, or Leyden jar, or the system of two conducting spheres separated by an insulator which Faraday investigated, and with which he proved that a certain period of time must pass before the insulator absorbs a complete charge of electrical energy. The phenomenon of retardation of signals, owing to the preliminary filling-up of the gutta-percha with electrical energy, first became practically noticeable in the Anglo-Dutch cable, which was 110 miles long.

In his first paper *On the Theory of the Electric Telegraph*, Thomson elucidated one of the most important properties of long cables, a knowledge of which is essential to successful operation. He showed that if a sharp electrical impulse is given to one end of a long cable, only a small part of its energy reaches the other end almost instantaneously. The impulse gradually spreads out into a wave whose beginning reaches the other end very quickly, but whose crest arrives only after a leisurely and almost imperceptible rising, and then fades away. The observer at the receiving end sees only a gentle rise and fall, without the definition necessary for signalling. Thomson showed that the magnitude of this phenomenon depended on the combined value of the electrical capacity and resistance of the cable. As each of these was directly proportional to the length, the

effect increased as the square of the length of a cable. If the retardation in a cable one hundred miles long were one-tenth of a second, then that in two thousand miles of the same sort of cable would be four hundred times as great, or forty seconds. Any system of signalling that depended on the observation of the crest of a wave could at the best send only one sign in forty seconds. As a single letter had to be represented by perhaps two or three signs, the delay destroyed the practical value of the cable. The proprietors could not earn profits if the transmission of one telegram was to occupy the cable for the whole of one day. Evidently the retardation could be reduced by reducing the resistance and the capacity, by increasing the thickness and purity of the copper wire, and increasing the thickness of the gutta-percha insulating material. In 1856 the promoters of the Atlantic cable enterprise decided they were ready to start construction. J. Brett, Charles Bright, Cyrus Field and O. E. W. Whitehouse formed a company with a capital of £350,000. The directors were elected by the vote of shareholders, and were to receive no remuneration until the shareholders had been paid a dividend of ten per cent. The Scottish shareholders nominated Thomson as a director. The board appointed Bright as chief engineer, Whitehouse as electrician and Field as manager.

Thomson's status was the same as that of the other directors. He had no authority over Bright, Whitehouse and Field in technical questions. They were at liberty to accept or decline his informal technical advice. In the early years of his interest in cables Thomson received very little money in spite of immense labour, and had the mortification of being almost unable to prevent Whitehouse from erroneous procedure through ignorance of electrical theory. When the company officially started construction it found that the preliminary committee had already sent specifications for the cable to contractors, and, to save time, had ordered manufacture to begin. Bright and Thomson found that the dimensions of the cable were much smaller than theory would have recommended, but the contract

could not now be cancelled. The importance of resistance led Thomson to investigate the conductivity of copper. He brought his students into a general investigation of specimens of commercial copper, and found that their conductivity varied enormously, and that a small percentage of impurity could reduce conductivity by 30 or 40 per cent. The cable was being manufactured in twelve hundred pieces, each two miles long, and the conductivity of the copper cores of the pieces varied enormously. Thomson's investigation of cable copper had several very important consequences. It led to the foundation of the first industrial laboratory for testing materials, and to the study of methods and units of measurement. When Thomson by determined opposition managed to have a clause specifying the conductivity of copper put into future cable contracts, the contractors at first said its fulfillment was impossible. Thomson's work had great influence on the standardization of engineering materials, and on practical measurement, and thus became an important preliminary to the industrial method of mass-production, which can be applied only to uniform raw materials.

After the cable had been manufactured it had to be laid. The British and the American Governments lent the ships *Agamemnon* and *Niagara* for this purpose. When the expedition was ready the electrician Whitehouse, who had been so recalcitrant to scientific advice, refused to sail owing to ill-health. At the request of the board, Thomson undertook the voyage. Whitehouse retained his position as electrician to the company, and Thomson went as a director and without salary or authority on technical matters. The cable broke after 330 nautical miles had been laid and the completion of the laying was postponed until the following year.

Thomson continued to work on the electrical and engineering problems of cabling. His mathematical study of electric conduction had shown that rapid cabling could be done only with very small currents, so he devoted his attention to the development of sensitive galvanometers. In 1858 he patented his famous Mirror Galvanometer, in

which large magnification was obtained by attaching a small mirror to the magnet suspended within the coil. A ray of light was reflected from the mirror on to a scale, thus providing a pointer of vast length and no weight. It is said that the idea of using the reflecting mirror occurred to him at a moment when he had happened to notice a reflection of light from his monocle.

In 1858 the cable-laying expedition started out again. Whitehouse again refused to sail, and Thomson was asked to supervise the testing-room on the *Agamemnon*, unpaid. He was not provided with resistance coils and other instruments for which he had asked. When the *Agamemnon* had been at sea for two days, she encountered a tremendous storm, that lasted for eight days. Two hundred and fifty tons of cable had been coiled on the upper deck. As the ship pitched and rolled, the heavy coils strained the deck, and made openings an inch wide between the planks. The electrical testing-room was flooded, members of the crew were injured, the coal broke loose, and portions of the cable became tangled. After the *Agamemnon* had been at sea for over a month, and numerous breakages had been repaired and immense difficulties had been overcome, completion of the laying approached as the ship neared Ireland. A junior member of the electrical staff described afterwards in the *Sydney Morning Herald* how on August 3rd, as the ship entered shallow water, Thomson entered the testing-room, "evidently in a state of enjoyment so intense as almost to absorb the whole soul and create absence of mind." On August 5th, the end was landed, and at five minutes to four Thomson sent the first electrical signal from Europe to America, receiving the reply five minutes later. The electrical operation of the cable was now handed over to Whitehouse.

The successful completion of the cable was celebrated with enthusiasm throughout Britain and America. Immense public expectation had been aroused by the achievement. But the expectation was rapidly disappointed. During the first week of Whitehouse's operation no messages were sent through the cable. He had discontinued the use of

Thomson's galvanometer and introduced his own patented instruments at the Irish end. They were designed on the mistaken principle of sending powerful currents through the cable. As Thomson had shown years before, sensitive detectors, not powerful currents, were the solution to the electrical problem of cabling. The directors and the public became alarmed at the absence of further communication. After a week the use of Thomson's instruments was partly resumed, and on August 13th the first clear messages were exchanged. But the Queen's cablegram of ninety-nine words had required sixteen and a half hours for transmission from Ireland to America, whereas the same message was returned from Newfoundland, where they were still using the old instruments, in sixty-seven minutes.

Whitehouse was suspended and Thomson was put in charge.

After he had investigated the situation, Thomson was now disposed to protect Whitehouse. He reported that he had "been conducting his own proper business in a thoroughly efficient and successful manner" and that "he was one of their most devoted officers." The board replied that Whitehouse had spent £12,000 of the Company's funds on instrumental investigation, without proper success, and that the successful instruments had been invented in the professor's study with his own resources and at small expense. Thomson still contended that the company owed much to Whitehouse, and said that his preliminary work had convinced the public that an Atlantic cable was possible. Whitehouse had a strong personality and considerable but limited knowledge. Like men of his type, he had a passionate belief in his own ideas, which gave him an infecting enthusiasm and dominating power, but also made him blind to their defects. Thomson said that the existence of the Atlantic telegraph was largely due to him.

The cable continued to work a little for about a month and then broke down almost entirely. Fierce public controversy started and Whitehouse wrong-headedly supported his opinions and actions, and seemed to lose his judgment of fact. Thomson had to reply to his inaccurate

statements and behaved with admirable forbearance. Thomson still struggled to make the cable work. While experimenting with it he discovered the phenomenon of an electric valve, through which a current passes more easily in one direction than in the other. The construction and insulation of the cable were defective. Silvanus Thompson states that even the Prince Consort was interested in the problem of insulation. He suggested to a director of the Atlantic Telegraph Company that the best plan would be to enclose the copper wire in a flexible tube of glass throughout its entire length. On being told that this was impracticable, he took down a volume of the writings of *Petronius Arbiter* to prove that flexible glass was a known substance.

Though the first Atlantic cable was not a practical success, it had proved that long-distance cabling was possible.

Thomson's connexion with the remarkable venture added a public to his scientific fame. At the age of thirty-five, after a dozen years of almost unparalleled fertility in pure scientific research, he became one of the best and most famous engineers of the time. His part in the voyages of the *Agamemnon* was a wonderful extension in his mastery of life.

Preparations were gradually made for the construction of a new cable. The problem was no longer a romantic venture. A solution was known to be possible, so every aspect was submitted to methodical preliminary investigation. By 1865 a new and vastly improved cable had been made. In July Thomson set sail once more on a cable-laying expedition. The entire cable was taken in the *Great Eastern*, a ship of 22,500 tons, which at that time could not be managed commercially on account of its size, and consequently lay idle, a bravura of nineteenth-century capitalism.

Again the expedition was not immediately successful. The cable broke in the mid-Atlantic, so the *Great Eastern* returned. The company suspended operations for the year and boldly ordered another cable. In 1866 Thomson

sailed for the fourth and successful cable-laying. The Irish end was landed in July and the cable immediately worked satisfactorily.

A few months later the *Great Eastern* sailed again, to retrieve the cable lost in the previous year. Its end was found and a new length spliced on, and the laying continued to Newfoundland. So at the end of 1866 the company found itself in the possession of two sound Atlantic cables! In the same year Thomson was knighted for his contribution to cable engineering and received the freedom of the city of Glasgow.

He had for years studied the problem of the automatic reception of cable signals. How could a galvanometer be made to write the messages it received from the other side of the Atlantic without being watched by an observer? In 1867 he patented a device which could accomplish this task. After three more years of detailed research a practicable model was produced. This was his famous Siphon Recorder. A very fine glass tube, of the sort used in vaccination, was bent and arranged as a siphon, one end of which was immersed in an ink-bottle and the other was held over a travelling paper tape. The light siphon was attached to a coil connected with the cable and suspended between the poles of a powerful magnet. When the coil received a current from the cable it twisted owing to the reaction against the magnetic field, and moved the end of the glass siphon to and fro over the travelling tape. The ink in the siphon was electrified by a small machine, and in virtue of its electrification was drawn towards the unelectrified paper, so that it spurted out in a fine spray. As the siphon moved to and fro, a wavy line was traced by the spray on the tape and could be made to convey signals. He had accomplished the invention of a pen that wrote without friction. Incidentally, he had invented the "moving coil" galvanometer.

His application of an electrified spray was perhaps due to his studies of atmospheric electricity, in which he became interested while experimenting with Joule on the expansion of gases in 1856. His students discovered later

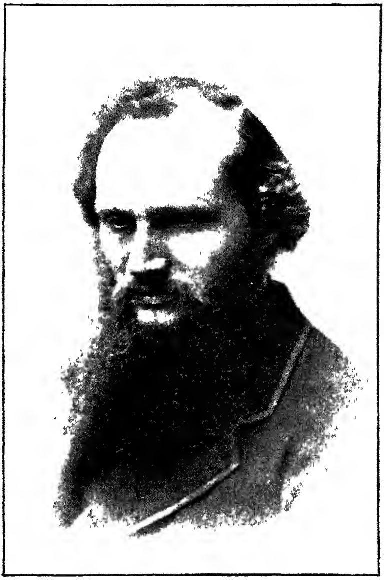
that the steam issuing from the funnel of a locomotive was negatively electrified, whereas that from the safety-valve was positively electrified.

During these years of prodigious toil, Thomson's private life was not entirely fortunate. In 1852 he had married Margaret Crum, a cultivated, but very delicate woman, who was a sick wife even during their honeymoon in Sicily. For eighteen years she lived on as an invalid. Thomson patiently and affectionately cared for her. After she had died in 1870, Helmholtz remarked how his domestic life seemed disordered without her.

On the Christmas Eve of 1860 Thomson fell and broke his leg while playing the ice-game of curling. It was not properly re-set and gave him a permanent limp.

Both of these misfortunes may have increased his appetite for work. The delicacy of his wife may often have prompted him to relieve the congestion of abnormal control by prolonged bouts of work, and his lameness may have increased his addiction to work by removing some of the possibilities of distraction. The physical disabilities of his wife and himself may also have increased his desire for wealth as a means to comfort. It is said that he secured his early patents in order to obtain money for his laboratory and experimental researches. His pursuit of wealth certainly increased and may have been stimulated by his disabilities, besides his desire to establish his success according to the conventional money standard of capitalism.

The large income derived from his galvanometer and other patents enabled him to purchase in 1870 a large sailing yacht of 126 tons, named the *Lalla Rookh*. Henceforth, he devoted much time to sailing. He became interested in the problems of navigation and invented improved forms of ships' compasses and sounding apparatus. He also studied the theory of the tides and adopted his brother's calculating machine to the prediction of the height of the tide at any place and date, given certain observational data. James Thomson's calculating machine was the forerunner of the remarkable machine recently constructed at the Massachusetts Institute of Technology by Dr. Bush,



SIR WILLIAM THOMSON, 1870

(Macmillan & Co., Ltd., from "The Life of William Thomson, Baron Kelvin of Largs")

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and the machine under construction at Manchester University by Professor Hartree. G. H. Darwin has written that his own work on the tidal theory of the evolution of the solar system was entirely inspired by Thomson's investigations of tidal theory.

While on the cable-laying voyages of the *Great Eastern*, Thomson had noted how the iron hull had interfered with the ship's compass. When ships had wooden hulls simple compasses had served quite satisfactorily. But the introduction of iron hulls, and especially iron armour on battleships, had shown that the indications of a simple compass could become dangerously misleading in the presence of so much iron. Thomson applied the results of Archibald Smith's investigations of the theory of the compass. It was necessary to make the compass insensitive to the disturbances of local masses of iron, and yet remain sensitive to the earth's magnetic field. Thomson arranged that the compass card should be very light, and should be directed by a widely-distributed group of little attached magnets. The lightness of construction reduced the friction at the pivot, and the distribution of the mass of the magnets gave a long period of vibration. The disturbing forces due to the iron hull are screened by placing near to the compass iron masses whose effects are already known. The old compasses with large magnets needed very large masses, whereas the new light compass worked satisfactorily with much smaller ones.

After considerable opposition from conservative officials, Thomson's compasses became generally used in the British Navy, largely owing to the vigorous advocacy of the late Lord Fisher.

He invented an ingenious gauge for sounding which consisted of an inverted closed tube, coated on the inside with silver chromate. As the gauge sank, the water pressed the air up the tube. After the tube had been rehauled to the surface, the maximum depth could be deduced from the height of the water-mark registered by the silver chromate. He devised a system of sounding with sinkers lowered by thin piano wire which offered

little resistance to the water. With this method, soundings could be taken without stopping the ship. Previously, sounding could not be done accurately without stopping the ship, owing to the displacement from the vertical by the resistance of the water, of the hemp rope bearing the sinker. Shipmasters often risked neglecting sounding with the old apparatus, in order to avoid the delay and trouble of stopping the ship.

Thomson capitalized the results of his navigational studies on his leisure yacht by a series of profitable patents. He became profoundly respected by seamen and business men, besides scientists.

His inventions were not restricted to appliances for practical use. The mirror galvanometer created a new standard of delicacy in electrical measurements. He invented a number of electrical measuring instruments, especially electrometers, that enabled electrical forces to be measured by direct comparison with other sorts of forces, such as weight. If an electrical force is measured in terms of another electrical force, the experimenter is always dependent on the safe preservation and invariability of the latter unit. If the unit is lost or an inaccurate copy is used he cannot be confident of his results. In his absolute electrometer Thomson arranged that the electrical force is measured directly by the weight required to balance it. He was thus able to measure the force in terms of the unit of mass, which, with length and time, is one of the three fundamental or absolute units.

With his quadrant electrometer an electrified needle was suspended over a metal disc divided into four quadrants. Opposite pairs of quadrants are connected together and the pairs respectively to any pair of objects, such as the earth and a charged sphere, whose difference of voltage is to be measured. The pairs of quadrants attract the needle and turn it on its suspending fibre. By twisting the screw which holds the fibre, the needle can be restored to its original position, through the torsion of the fibre. The size of the angle of twist is a measure of the voltage difference.

Thomson ingeniously applied an electrometer to measure

the difference of voltage between a conductor and the air, by arranging a stream of fine particles to leave the conductor. If the conductor has the higher voltage the particles will be positively charged, and if the lower, negatively. By connecting the conductor to the electrometer the change in its voltage, and hence the voltage of the air near to the stream of particles, can be measured. In later designs the electrometer registers a spot of light on a sensitive paper, and a continuous record of the change in atmospheric voltage can be made. During a day the variations may be as large as 1,000 volts.

The magnitude of an electric current is legally measured by an Ampère balance. The form devised by Thomson consists of two movable coils attached to the ends of the beam of a balance. Above and beneath these are fixed coils. All of the coils are connected in a circuit so that the coil at one end of the beam is pulled down and the coil at the other end is pushed up. The beam is restored to the horizontal position by adding weights to the raised end. The size of the current passing through the circuit can be calculated from the amount of weight that has to be added in order to restore equilibrium.

The delicacy of electrical measurements is now one of the most noticeable features of science and engineering. This development owes much to Thomson's initiative in devising sensitive instruments that measured according to null methods, i.e. by methods in which forces are measured by equilibration against other forces, so that absence of movement is the test of equality. As absence of movement can always be detected more exactly than the degree of an increase or decrease in strength, null methods are desirable for accurate measurement of forces.

Though his instrumental inventions had a very important influence on the development of science and electrical engineering, Thomson was not a perfect inventor. In a letter to Faraday concerning electrometers, written in 1860, he remarks that he had "a great deal of trouble in making the glass fibre suspension, having very little skill of hand, so that what would be easy and short to others costs me a

great deal of time and trouble." He had not the perfect inventor's combined mastery over principle, materials and construction. Most of the instruments that he designed were like the materialization of theoretical propositions; as if the theorems of original memoirs had risen up from the pages of learned journals and taken flesh. They seemed to have been brought into existence through the violent clarity of Thomson's conceptual grasp. In design they were embodiments of propositions in the mathematical theory of electricity, rather than machines created by a man who was their understanding friend and saw things from their point of view. Many of Thomson's instruments reflected the elegance of his mathematical physics, but could not easily be manipulated. He invented with remarkable and influential success, but most of his instruments lacked the engineering elegance of the finest inventive genius.

His interest in measurement led him to suggest the formation of a committee on Electrical Standards, after the reading of a paper by Charles Bright and Latimer Clark, on the terminology of electrical units, at the meeting of the British Association in 1861. Bright and Clark had suggested the names "galvat" for a unit of current, "ohma" for electromotive force, "farad" for quantity and "volt" for resistance. The first members of the committee were Williamson, Wheatstone, Thomson and W. H. Miller, with Matthiessen and Fleeming Jenkin as reporters. After six years of labour the committee fixed the features of what is now the System of Electric Units.

Thomson recommended Weber's method of determining the unit of resistance by spinning a coil in the earth's magnetic field. The unit was named the "ohm." At that time the value of the ohm was not known correctly. Distinguished experimenters, such as Wheatstone, had previously adopted the resistance of an arbitrary piece of wire as their unit.

The theory of absolute measurements of electrical and all other forces in terms of the fundamental units of mass, length and time was worked out by the great mathematician Gauss, and his pupil, Weber. Gauss had become interested

in the observation of magnetic phenomena and terrestrial magnetism. He built an iron-free observatory at Göttingen for this purpose. His powerful generalizing mind made him dissatisfied with makeshift methods of measurement, so he discussed the general theory of how all measurements were related and could be expressed in terms of a minimum number of chosen units, such as mass, length and time. Gauss' invention in science, as distinguished from mathematics, is shown by his creation of the first electric telegraph, for sending messages from the observatory to a laboratory some distance away.

The theory of measurement in terms of the absolute units, given by Gauss and Weber, now required to be socialized and adapted to the use of the engineer. Thomson and the British Association committee had a major part in the accomplishment of this task.

Practical units must be of a size convenient to rapid and accurate calculation. The currents met with in practice should be expressible in terms of convenient numbers and units. For instance, a householder would be confused if he had to pay for electricity by the billion units. The current and pressure of electricity passing through a lamp should be expressible in easy numbers of units, such as one-tenth of a unit of current, or one hundred units of electrical pressure.

In 1862 the British Association committee adapted as a practical unit of resistance a quantity one thousand million times the absolute unit, and named it the *Ohm*. International congresses at Paris in 1881 and Chicago in 1893 agreed to the representation of the ohm as equivalent to the resistance of a column of mercury 106·300 centimetres long and weighing 14·4521 grammes at the temperature of melting ice.

The practical unit of current was named the *Ampere* and was one-tenth of the absolute unit. It was agreed as equivalent to a current which deposits silver in an electrolytic cell at the rate of 0·001118 grammes per second. The practical unit of electrical pressure was named the *Volt*, and was defined as the pressure necessary to send a current

of one ampere through a resistance of one ohm. When the British Association committee began its work in 1861, laboratory research workers did not know the value of an ohm within an accuracy of a few per cent., though engineers in electrical factories had been forced to develop a far higher standard of accuracy owing to the necessity for economy in materials. For instance, if 4 per cent. too much copper were used in an electrical construction because the engineers could not measure resistance within an accuracy of 4 per cent., four thousand pounds sterling would have been wasted in an expenditure of one hundred thousand pounds for copper conductors.

Many experimenters were engaged by the British Association committee in redeterminations of units of measurement. Exact measurement grew into an independent branch of science, requiring its own research laboratories. In Britain this requirement was met by the foundation of the National Physical Laboratory in 1900. As the mercantile society of the seventeenth century required accurate astronomical knowledge for the safe conduct of navigation, and founded the Greenwich Observatory to meet its need, the machine industrialism of the nineteenth and twentieth centuries met its need for accurate measurement of the materials of its manufactures by the foundation of the National Physical Laboratory.

Thomson made yet another first-class contribution to the adaptation of science to the needs of a new type of social organization. Nineteenth-century industrialism expected the experts it employed to know how to apply mathematics to the problems whose solutions it required. It wanted the solution of difficult problems in the design of complicated engines and electrical machinery. It desired the use of mathematics in a particularly concrete manner, in which the physical meaning of every stage of a mathematical calculation could be visualized. Thomson's mathematical methods were wonderfully suited to meet this demand. He preferred to use Cartesian co-ordinates which have the most direct reference to the environment. They are the simple idealization of the three dimensions of space, and

the oldest and most familiar of co-ordinate systems. With a taste for Cartesian methods and clarity combined with concreteness of style, he was the natural exponent of the application of advanced mathematics to the new theoretical problems in engineering, presented by the refinement of the design of heat engines and electrical machinery.

The physico-mathematical interests of the eighteenth century had been inspired primarily by the problems of astronomy, and had led to the composition of the magnificent works of Lagrange and Laplace. Students with an aptitude for mathematical physics accepted these works as their guide and absorbed their attitude. By the middle of the nineteenth century society was requiring mathematical physicists with another attitude. The generalized methods of Lagrange and the comprehensive investigations of Laplace belonged to the mature development of dynamical astronomy, a subject that had been studied intensively for over a century.

In the early part of the nineteenth century a vast number of new and strange natural phenomena had been observed and began to require mathematical description. The new and at first ill-defined and often apparently independent facts required very concrete methods of treatment. Their raw, uncouth nature tended to wreck the symmetry of general equations. They could be seized best by short, uniformly clear descriptions of each isolated phenomenon. Thomson's style of short terse papers was suited to their treatment.

Thomson and his friend Tait, who had been appointed professor of natural philosophy at Edinburgh, decided to write a *Treatise on Natural Philosophy*, which would expound the mathematical physics suitable to the contemporary demand. They expounded the science of mechanics, unconsciously, from the standpoint of an ideal engineer who was a master of mathematical physics. Maxwell wrote: "The credit of breaking up the monopoly of the great masters of the spell, and making all their charms familiar in our ears as household words, belongs in great

measure to Thomson and Tait. The two northern wizards were the first who, without compunction or dread, uttered in their mother tongue the true and proper names of those dynamical concepts, which the magicians of old were wont to invoke only by the aid of muttered symbols and inarticulate equations. And now the feeblest of us can repeat the words of power, and take part in dynamical discussions which a few years ago we should have left to our betters."

Thus Thomson and Tait accomplished, on behalf of the educated leaders of the industrial bourgeoisie, the conquest and assimilation of the mathematico-physical culture of the mercantilist class.

The influence of the result of this class-struggle in one of the most elevated regions of human endeavour spread down into the teaching of elementary mathematics. Thomson's pupils, Ayrton and Perry, led the movement for the teaching of "practical mathematics." They explained that the new class of technician, brought into existence by machine industry, wanted a knowledge of mathematics which would be of practical use to him in his job. They contended that these men should be taught the sort of mathematics which they would find useful. Mathematics was taught at the grammar schools and universities as if it were to be the cultural accomplishment of certain members of a leisured class, and not a technical equipment which would enable its possessors to earn a living. Ayrton and Perry desired, more or less consciously, to conquer elementary mathematics for the large class of the skilled workmen and technicians. Silvanus P. Thompson, the biographer of Thomson, was a leader in the same movement, and wrote the famous book, *Calculus Made Easy*, which was an attempt to wrest the exclusive knowledge of the calculus from the classes educated in grammar schools and universities, and place it at the service of the classes of skilled workmen and technicians.

The *Treatise* of Thomson and Tait had the limitations of its nature. It was entirely unphilosophical. It contained very little discussion of the fundamental conceptions such as mass, motion, relative motions, and how the human

intelligence had derived them. Readers were supposed to know without instruction the nature of these things. The authors assumed, probably unconsciously, that the world is naturally to be comprehended in terms of an engineer's conceptions.

The second edition of the *Treatise* was reviewed by Clerk Maxwell in one of his last writings. He commented on the authors' unsatisfactory discussion of the concept of mass. The parent of twentieth-century physics appreciated the limitations besides the excellencies of the school of Thomson and Tait.

The writing of his share of the *Treatise* gave Thomson a vast amount of hard work. He used to carry proofs with him in trains and ships, and worry the printers by re-writing and making long additions to the copy that had already been set. The *Treatise* was written while Thomson was occupied by the laborious preparations for the second and successful Atlantic cable, his professional duties, and an extraordinary number of additional scientific activities; and published in 1867.

In 1873 he visited Madeira in connection with the laying of a South American cable and met Miss Frances Blandy, the daughter of one of the chief landowners on the island. In 1874 he returned to Madeira and married her. Miss Blandy became the most capable and affectionate manager of Thomson's household. For the rest of his life she cared solicitously for his well-being.

Thomson built a large country house at Largs, near Glasgow, for the scene of his second married life. The design was largely his own and ingenious, but hideous. Owing to his engineering feeling the building was slightly functional in intent, but he had the outside covered with masonry in the Scottish baronial style. His house was fitted with electric light in 1881, and was the first, or a very early, domestic house to be lighted by electricity.

Thomson was interested in politics. During the first half of his life he followed his father's Radical tradition, and enthusiastically supported Gladstone's earlier campaigns. He considered an invitation to contest the Parliamentary

seat for the Universities of Glasgow and Aberdeen, but decided he could not afford the time for the adequate performance of parliamentary duties, so he declined. In 1871 he regarded war as a relic of barbarism and believed it would become as extinct as duelling. In 1879 he wrote that "it must not be supposed that those who do not want to fight are devoid of patriotism." But his Radicalism was upset by Gladstone's advocacy of Irish home rule. As an Ulsterman, he was unable to appreciate the merit of the proposed extension of the principle of self-government and political freedom. He became an ardent Liberal Unionist, and Imperialist, and admirer of Lord Rosebery and Lord Salisbury. This political evolution was an important factor in his elevation to the peerage in 1892. Lord Salisbury was the prime minister of the day, and wrote that he was sure Thomson would strengthen their party in the House of Lords. By 1904 he had become converted to the idea of free trade "all over the British Empire, Colonies, mother-country, and all; but thinks a small protective tariff against foreign goods would be beneficial." He saw no reason to exempt food imports from taxation, as the costs of housing and clothes bear more heavily on the poor. "More than once he expressed the wish to see party government disappear, and to have a cabinet in which all parties should be represented."

One of Thomson's nieces married J. H. Gladstone, who succeeded Faraday as Fullerian professor of chemistry at the Royal Institution. Gladstone's daughter Margaret was a favourite of her grand-uncle during his later years. She astonished the Thomson family by marrying Ramsay Macdonald, "a very great surprise to us all and a bit of a shock, but it in no way broke the warm friendship and the new-comer was soon admitted to our family circle. . . . Lord Kelvin remarked on several occasions what a nice young man Ramsay was and what a pleasant talk they had had"; so Agnes G. King writes.

The Thomsons were much influenced by family tradition, largely created by their father, the professor of mathematics, who had been a remarkable personality. The old professor's

LORD AND LADY KELVIN IN CORONATION ROBES
(*Mr. D. T. King*)

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textbooks continued to be used in Scottish schools for generations after his death. His *Arithmetic* passed through nearly one hundred editions.

Perhaps Ramsay Macdonald's ideas of a National Government of leaders from all parties was adopted from Thomson, partly through a desire to align himself with the family tradition and complete the assimilation into the upper classes, which seems to have been one of the most important motives of his career. Many will remember Macdonald's speech at the Queen's Hall meeting in 1931, during the Faraday centenary celebrations. He found time to come to refer to his interest in Faraday through his connexion with J. H. Gladstone and Thomson. He was, no doubt, expressing a family duty.

Thomson's evolution from a Radical into an Imperialist was parallel to his evolution from a professional into an industrial scientist. In later years, when he had added the occupations of consulting and manufacturing engineer to his professional work, he adopted the political views characteristic of the manufacturers of machinery and metal goods.

His religious practice exhibited an evolutionary curve parallel to those of his politics and occupations. In early life he attended the services of the Scottish Free Church and in later life those of the Scottish Episcopal Church.

The jubilee of his service to the University of Glasgow was commemorated in 1896 by a brilliant celebration attended by over 2,000 guests, including eminent scientists from all parts of the world. In his reply to this overwhelming demonstration of regard, Thomson astonished his audience by commenting that "one word characterizes the most strenuous efforts for the advancements of science that I have made perseveringly during fifty-five years; that word is *Failure*. I know no more of electric and magnetic force, or of the relation between ether, electricity, and ponderable matter, or of chemical affinity, than I knew and tried to teach to my students of natural philosophy fifty years ago in my first session as professor. Something of sadness must come of failure; but in the pursuit of science,

inborn necessity to make the effort brings with it much of the *certaminis gaudia* and saves the naturalist from being wholly miserable, perhaps even allows him to be fairly happy in his daily work."

This passage proves that Thomson was aware of his own tragedy and that he was afflicted by the divine discontent, and was a truly great man.

After a long career of almost unparalleled fertility and honour, in which he published over six hundred papers on original research and scientific discussion, and patented seventy inventions including several of the most important produced in the nineteenth century, was honoured by two hundred and fifty academies and societies and accumulated a large fortune, he found the word failure most suited to the description of his career. What did he mean? Most of his hearers thought that his speech was a simple expression of modesty.

The theme of Thomson's intellectual life was a search for a comprehensive theory of material phenomena, the construction of "a great chart, in which all physical science will be represented with every property of matter shown in dynamical relation to the whole." He assumed that the complete description of material phenomena was to be derived from the common objects of experience; atoms must behave according to laws that had been derived from the observation of quantities of matter comparable with the size of the human body. He supposed that physical science was to be fitted exactly into the engineer's concepts of nature. With lifelong persistency and with the expenditure of prodigious intellectual power, Thomson sought to force the physical concept of nature into the moulds of the engineering imagination. His early paper on the analogy between the propagation of electrical forces and the transmission of strains in an elastic solid, contained his most brilliant and inspiring success in that endeavour. But he could not be satisfied with a partial analogy; he desired a complete analogy with engineering ideas. Unlike Maxwell he could not accept the helpful part of the analogy and not be bothered too much by the inconvenient part. He

wanted his engineer's concept of the universe to be exact and complete. Owing to this attitude he could not comfortably accept Maxwell's electro-magnetic theory of light or fully appreciate the importance of the Michelson-Morley experiment, or the phenomena of radioactivity. He was fascinated by Helmholtz's brilliant mathematical discussion of vortex rings, and hoped to explain the permanency of atoms and their possession of structure, in terms of vortical properties. The idea of vortex atoms grew, under the cultivation of others, into the concept of the planetary atom, consisting of a nucleus surrounded by revolving electrons. Thomson was able to contribute to the evolution of that aspect of the modern concept of the atom, which is describable in the visual images of an engineer.

The failure to accomplish the object of his life's search for a comprehensive theory contained, in fact, his greatest contribution to science. He had submitted the engineering concepts of nature to a long and powerful intellectual hammering, in the attempt to shape them into a complete description of physical phenomena. His failure proved that the future concepts of physical nature were not to be found in that direction, and that another path must be found. His successors were relieved from the temptation to follow the path of the engineering imagination. His work warned Planck, Rutherford, Einstein and Bohr from that ultimately misleading path. The example of Thomson's titanic, but ineffectual struggles prevented succeeding geniuses from repeating his mistake. This aspect of his work is illustrated by his *Baltimore Lectures*, delivered in 1884 and published in 1904. His audience consisted of twenty-one American professors and his theme was *The Failures in the Wave-Theory of Light*. The unconscious theme of this extraordinary book was the failure of the engineering concepts as a basis of scientific theory.

Thomson's belief in the finality of engineering ideas was related to the ideology of industrialism.

Nineteenth-century capitalists, like all other governing classes, believed that the structure and ideology of their social organization were eternal. A class that gained power

and governed by the exploitation of engineering tended to believe that the engineer's mode of conceiving nature is ultimate. Thomson's expression of this belief explains his tremendous social significance. He was the leading symbol of the scientific ideology of the British nineteenth-century governing class. His friend Helmholtz occupied a similar position in the cultural system of the governing class of nineteenth-century Germany. They resembled each other in the immense range of their scientific knowledge, their industry, their public fame, their concreteness of thought and insensitiveness to the intimations of the ideas that would become fundamental in future science.

Thomson had remarkable stamina and concentration. He could dictate to three secretaries at once and sleep when he wished. He could manage large teams of scientific assistants. He preserved his clarity of mind and his leading ideas to the last as he demonstrated, a few months before his death, in his remarkable contribution to the discussion of the structure of the atom, at the British Association meeting of 1907. After Rutherford had described the implications of recent research on radioactivity, Thomson stated with singular exactitude his belief that "it seems almost certain that there are many different kinds of atom, each eternally invariable in its own specific quality, that different substances such as gold, silver, oxygen consist each of them of atoms of one invariable quality, and that every one of them is incapable of being transmuted into any other."

He was an intellectual colossus who saw one-half of the aspects of material nature with unsurpassed clarity and power, but was blind to the other half. His great personality was an expression, in the realm of ideas, of the power and of the blindness of capitalism.

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V

JAMES CLERK MAXWELL

1831–1879

JAMES CLERK MAXWELL

1831–1879

MAXWELL is universally accepted as the greatest theoretical physicist of the nineteenth century. In the words of Max Planck, the senior leader of twentieth-century theoretical physics, "it was his task to build and complete the classical theory, and in doing so he achieved greatness unequalled. His name stands magnificently over the portal of classical physics, and we can say this of him: by his birth, James Clerk Maxwell belongs to Edinburgh, by his personality he belongs to Cambridge, by his work he belongs to the whole world."

All geniuses create by the application of imagination to knowledge. The dramatist applies his imagination to his knowledge of humanity, the lyricist his imagination to the description of nature, and the scientist to his concept of nature. The various sorts of creative activity differ in their subject matter only; the same sorts of imagination may be found in all of them. In Davy's earlier Bakerian Lectures there are passages which show the operation of a flame-like impetuous imagination resembling Marlowe's.

A parallel to the quality of Maxwell's imagination may be found in Shakespeare. Both of these supreme geniuses achieved considerable fame in their own time, but both were only partially understood. Milton wrote of "sweetest Shakespeare, Fancy's child." This colossus loved and misunderstood him, as the British nineteenth-century colossus Thomson admired and misunderstood Maxwell.

Indeed, it is doubtful whether Maxwell understood himself. He was deeply religious. His continuously active cultivation of religion, like Faraday's, had a very important relation to the course of his intellectual life. It preserved him from some of the philosophical errors of his time, for the rejection of philosophy is a Spartan method of avoiding philosophical errors, and also, as a substitute for professional philosophical study, prevented him from formulating a philosophy by which he might explain his ideas to others. By his submission to religion Maxwell saved himself, but not his generation, from certain philosophical errors. The religious decisions of Faraday and Maxwell were inelegant, but effective evasions of social and intellectual problems that distracted and destroyed the qualities of the works of many of their ablest contemporaries. Shakespeare faced the deepest intellectual problems, but evaded social problems. Maxwell did not entirely evade social problems. He persistently lectured to working men in Cambridge, Aberdeen and London; and remarked that his proletarian audiences at Aberdeen often understood scientific ideas better than his university students. He was interested in F. D. Maurice's Christian Socialism.

He had a fascinating sense of humour. His lack of pomposity hampered appreciation during the culmination of a social process that had elevated business men into the ideal type. There are resemblances between Maxwell and Tchekhov. Both of them were subtle and humorous, died young, achieved fame during their lives and much greater fame after their deaths. Both were supposed during their lives to lack intellectual robustness and to be unsound in spite of their brilliance. They had a similar sort of alert sensitive intelligence. His inaugural lecture as first Cavendish professor of experimental physics was a brilliant description of the spirit that the new laboratory was to cultivate, but he was not quite confident that his delivery would be completely successful, so the lecture was delivered in an obscurely advertised place to a score of students. Later in the term his course of professional lectures on heat began. Adams, Stokes and the other

great figures of the university came to the first lecture of the course, under the impression that it was his inaugural lecture, and sat in the front row. They were much puzzled when the new professor spent most of the hour explaining to them, with solemn countenance but twinkling eyes, the arithmetical relations between the Fahrenheit and Centigrade temperature scales.

In his youth Maxwell's hair and beard were black, his eyes almost black, and his skin sallow. His features were beautiful and expressive. He rarely laughed, but his eyes twinkled when he was being ironically humorous. He had a tendency to speak in hyperbole which confused the simple and against which he struggled. When he was being ironical his voice often became husky, so that apprehension of his meaning became more difficult. His biographer, Lewis Campbell, has written that Maxwell's hyperbole was probably increased through bullying by a tutor, and at school. It was partly a psychological defence mechanism.

Like Davy, Maxwell had a taste and power for composing verse. Most of his compositions were light parodies on scientific subjects, the gambols of his phantasy after it had been unharnessed from struggle for new scientific concepts. His verse is a very valuable secondary expression of his mental characteristics. The nature of Maxwell's contribution to science is profoundly related to his social circumstances. He belonged to the class of small Scottish landowners, or lairds. His family had belonged to this class for three centuries and had produced several members of some note in Scottish history. His father enjoyed a small but secure income and in his youth desultorily practised law in Edinburgh; and as he grew older transferred his chief attention to the development of his little estate near Middlebie, in Dumfriesshire. John Clerk Maxwell had some of the best habits of his class. He reflected his security in independence of opinion and action. He employed his mental leisure in devising schemes for the improvement of his property and informing himself of the progress of science and technology. His wife died

when James was eight years old and he became almost a mother besides a father to his son. The remarkable sympathy that John Clerk Maxwell extended to his son helped to develop James' power of personal and intellectual sympathy. His interest in technology helped James to escape the ideology of the territorial class and become acquainted with the spirit of the culture of capitalist industrialism. If Maxwell had not accomplished this escape he could not have become an instrument of adaptation of British physical science to the needs of a new social order.

Maxwell's intellect had two qualities both of singular significance, clarity and apparent obscurity. In his own day, and until recently, the clear part of his mind was most esteemed. It contained the expression of the spirit of his own time. The obscure part is now seen to foreshadow the spirit of the succeeding time, and the wider knowledge possessed by the twentieth century has shown the meaning of its obscurities.

Maxwell could use engineering concepts for the development of science as powerfully as Thomson. He could describe the machinery of atomic behaviour with a clarity that seemed to provide his audience with personal introductions to the inhabitants of the atomic world. His mastery of the engineer's visual imaginative method of conceiving phenomena almost persuaded his contemporaries that he was one of the chief exponents of their scientific philosophy. But those who knew Maxwell personally could never feel quite convinced that he belonged to their school. Somehow or other, in spite of intellectual appearances, he was not sound. Their feeling was correct. As a member of the engineering school of scientific philosophy Maxwell was not sound. In spite of his very powerful use of the method and his frequent praise of it, he was quite ready to abandon it when it would not work. The degree of Maxwell's scepticism of the value of engineering concepts as a mode of developing science was not evident to his contemporaries, and probably not even to himself. When he got into philosophical difficulties, through trying to use engineering concepts for unsuitable purposes, he did

not tell the world of his trouble but retired into the privacy of his religious meditations. Hence the strength of the symbolical part of his imagination was concealed by his religion. The sort of imagination that made him the grandparent of the modern theory of matter, which does not employ the engineer's mode of imagination, was implicit in his scientific work but explicit only in his religious meditations. In this way Maxwell inspired the non-visual theories of the twentieth century while appearing a visualist to his contemporaries and immediate successors. Maxwell's writings, like Newton's, reveal an inspired caution of statement. In spite of Maxwell's prominent use of intellectual models he never came to believe that they were the reality beneath phenomena. He was never tied to them. Newton's marvellous mind also preserved him from similar errors, for he cautiously defined his mechanics so that it could not be upset by the quantum mechanics. He carefully stated that his mechanics of bodies was based on the assumption that the smallest parts obeyed the same laws as the wholes.

Maxwell's greatest achievement was his direction of the path of future research, through the reputation and power he had gained by his mastery of contemporary modes of scientific thought. He equalled the typical nineteenth-century physicists in their own modes and then indicated the path for the twentieth century, both in theoretical and experimental physics. His theory of electricity and magnetism led to the theory of relativity, his dynamical theory of gases contributed towards the quantum theory, and his scheme of work and methods for the Cavendish Laboratory, outlined in his inaugural lecture, led to experimental atomic physics.

The aspect of the culture of the nineteenth century expressed by physical science was fortunate to receive such a leader. Maxwell, too, was fortunate to live during a cultural period that was healthy and powerful enough to provide scope to his splendid genius. Though his contemporaries could apprehend only a half of his qualities, they did not fail in what was reasonably within their power and unconsciously helped their successors besides themselves.

The sufficient appreciation of Maxwell was one of the nineteenth century's worthiest achievements and a reflexion of the healthiest aspect of its culture.

James Clerk Maxwell was born in Edinburgh on June 13th, 1831. His ancestors included many notable persons. The family fortune had been made by John Clerk in Paris in 1634–46. He returned to Scotland and bought the barony of Penicuik. His son married the granddaughter of the poet Sir William Drummond of Hawthornden. Some of their descendants became eminent Scottish lawyers, who educated several of their sons in Leyden and other foreign cities. Maxwell's great-grand-uncle, John Clerk, was a friend of James Hutton, the founder of modern geology, and claimed to have invented the naval tactics by which Rodney won the battle of Dominique. Maxwell's great-grandfather, Sir George Clerk, had married his first cousin, Agnes Maxwell, the descendant of Drummond and the heiress of Middlebie, and had taken the name Clerk Maxwell. Thus the family now possessed the properties of Penicuik and Middlebie. The inheritance was arranged so that these properties could not be held together by the same son, so Maxwell's father inherited Middlebie and his Uncle George inherited the property at Penicuik. Sir George Clerk was for many years member for Midlothian and held office under Sir Robert Peel. He had a remarkable command over statistics and questions concerning weights and measures..

Thus Maxwell was descended from an inbred family containing many able persons, who included law, politics, geology, mining, statistics, poetry, music and strategy among their interests.

When the Middlebie property passed to John Clerk Maxwell it was in a poor condition. It did not even contain a house for the laird. While he remained unmarried John was not much interested in it and continued to live in Edinburgh, where he lived quietly but independently, practising a little law, but mainly interested in following the progress in science and technology. He was very fond of visiting every sort of factory and great buildings. In

1831 he published a paper on *Outlines of a Plan for combining Machinery with the Manual Printing Press.* With John Cay, who became his brother-in-law, he made many experimental attempts to construct a bellows that would deliver a continuous blast. Attendance at meetings of the Royal Society of Edinburgh was one of his chief pleasures.

When James Clerk Maxwell was in the most intense stage of his preparation for the Cambridge tripos examination he went to stay with a friend in Birmingham for a short rest. His father wrote that he should not fail to "view, if you can, armourers, gunmaking and gunproving—swordmaking and proving—papier mâché and japanning—silver-plating by cementation and rolling—ditto, electrotype—Elkington's works—Brazier's works, by founding and by striking out in dies—turning—spinning teapot bodies in white metal, etc.—making buttons of sorts, steel pens, needles, pins and any sorts of small articles which are curiously done by subdivision of labour and by ingenious tools—glass of sorts is among the works of the place and all kinds of foundry works—engine-making—tools and instruments—optical and (philosophical), both coarse and fine. If you have had enough of the town lots of Birmingham you could vary the recreation by viewing Kenilworth, Warwick, Leamington, Stratford-on-Avon, or such like." Campbell writes that James began with the glassworks.

This letter does not seem to confirm John Clerk Maxwell's reputation for indolence. It has a remarkable resemblance to the letter that Newton wrote to a young relative visiting Italy, in which the recipient is advised to notice all technological and geological phenomena without mentioning the things for which that country is more famous.

After the death of his mother in 1824 John married. His wife came from Northumberland. She was a Miss Cay, and had a sanguine active temperament. The Clerk Maxwells now became constructive. They resolutely improved their Middlebie property, and had stones and boulders removed in order to prepare land for cultivation. They had a small house built named "Glenlair," every detail of which was designed by themselves. Mr. Maxwell's

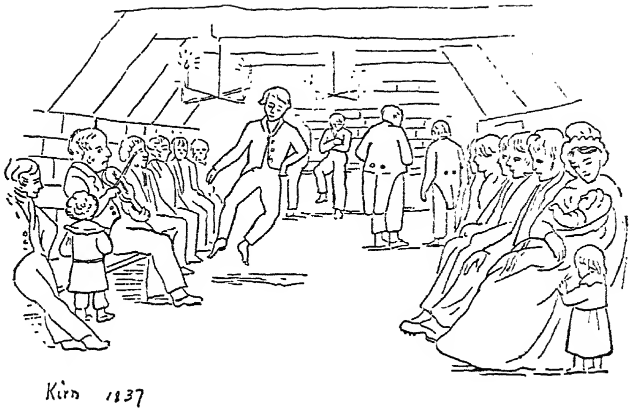
direct interest in things released him from conventionality. He designed the last for his own square-toed shoes and chose the leather for their manufacture. He cut out his own and his son's shirts, and drew the working-plans for the outbuildings of his house with his own hand. James Clerk Maxwell's biographer, Lewis Campbell, has remarked, with much insight, that the effects of John Clerk Maxwell's habits and characteristics are apparent in the construction of the Cavendish Laboratory. He would not have been incorrect if he had said that the effects of his habits and characteristics are apparent in the history of British and of human scientific culture. John Clerk Maxwell's interest in the development of the technology created by capitalism was an essential factor that helped James Clerk Maxwell to reform the study of physical science in Cambridge, and to adapt it to the needs of the culture of the new governing class of industrialists.

Before James was three years old the degree of his curiosity was noted in letters. In childhood he continually asked "What's the go o' that?" and if vaguely answered he reiterated: "But what's the *particular* go of it?"

It is recorded that Maxwell's oldest recollection was of lying on the grass before his father's house and looking at the sun and *wondering*.

His cousin, Mrs. Blackburn, was a talented draughtswoman and has left some delightful drawings of Maxwell in his childhood. From the age of six he is frequently depicted in intense observation or in making something. He had a very powerful memory and knew the 119th Psalm by heart when he was eight. From childhood he had a minute knowledge of the Bible and of Milton's works.

His mother died apparently from cancer in her forty-eighth year, when James was eight years old. His father now had to manage his life and accomplished the delicate work with wonderful understanding. It is interesting to note that the two great Scottish physicists of the nineteenth century were both motherless from their early childhood, and that their rearing was done by their fathers. Educationists might make an instructive study of the educational effectiveness



JAMES CLERK MAXWELL

Aged six, watching the Fiddler at a Barn Dance

(From the drawing by his cousin, Jemima. Macmillan & Co., Ltd.)

of the sympathetic masculine mind in contrast with that of the maternal feelings. It is possible that the development, and especially the early manifestation, of the geniuses of Thomson and Maxwell may have owed much to the thorough explanations of phenomena given to them by their educated fathers, and that they thereby received an intellectual grounding which they could not have received from affectionate and capable mothers without scientific knowledge.

James was very happy with his intelligent father at Glenlair. He was delighted by the various activities of rural life and playing in the stream by the house, and rambling over the country-side. He used to put frogs in his mouth to see them jump out. Among his toys he had a phenakistoscope, the primitive form of cinematograph invented by Faraday. Years afterwards he applied the visual effect of rotation in his invention of the colour top, and added lenses to the phenakistoscope, advancing it a stage nearer to its modern form as the cinematograph. He used it for showing pictures of the collisions of vortex rings, which was probably the first application of the cinematograph to the illustration of scientific phenomena.

He enjoyed the company of the children of his father's employees. While he played together with them heartily their difference in social class was not forgotten. The Maxwells referred among themselves half-humorously and half-seriously to their "*vassals*." Their attitude to their servants was as amiable as class difference permits, but fully grounded on that difference. Maxwell grew up with the intellectual habits of a member of the governing class, though he never lost the accent of the Scottish country dialect he had acquired from his playmates. His unconscious confidence in his power of government was shown in his remarkable intellectual legislation for the government of the Cavendish Laboratory. This confidence was developed by his direction of the games of the companions of his childhood. His sensitiveness would probably have prevented him from finding an opportunity for exercising leadership if he had been born a proletarian. Class advantage

provided him with an easy opportunity for learning the art of government, that he would probably never have gained if he had been born without it.

When his mother became ill and for about two years after her death, Maxwell was educated by a tutor who reported that his pupil was slow at learning. Maxwell's aunt presently discovered that the tutor had tried to bully knowledge into him, sometimes smiting his head with a ruler and twisting his ears until they bled. His father seems to have failed to notice these methods or accepted them as conventional. His biographer was convinced that "a certain hesitation of manner and obliquity of reply," which remained with him through life, were due to this early bullying. One of cousin Jemima's most fascinating drawings depicts James sailing in a tub in the middle of the pond, while his tutor is trying to hook him out with a hay-rake before an audience of his father, aunt, cousin, two playmate sons of labourers, four ducks and a dog.

The duration of James' oppression by the tutor seems to have been partly due to his own reticence. He could be quietly obstinate and probably wished to defeat him without external aid. Perhaps his feeling of class superiority also prompted him not to complain under the oppression of a half-educated member of a lower class. The tutor was probably a worthy youth who had learned some Latin in a rough school and did not know how to teach it by any other method. He must have been perplexed by the difficulty of the application of barrack-square methods to a child of a higher social class and of exceptional intelligence and character.

His father decided that other arrangements for his education must be made, and after much consideration placed him in the Edinburgh Academy, then a new school not yet twenty years old, very popular with the higher professional classes. He was ten years old and remained at the school until he was sixteen. During these years and the three following Maxwell lived at his aunt's in Edinburgh during term and spent his holidays at "Glenlair."

Though Mr. Maxwell had thought carefully about his

JAMES CLERK MAXWELL
Aged ten, being educated by his tutor

(After the drawing by his cousin Jemima, who holds the stick at the left. Macmillan & Co., Ltd.)

JAMES CLERK MAXWELL
Aged ten, arriving at his aunt's house in order to attend the Edinburgh Academy

(After the drawing by his cousin Jemima. Macmillan & Co., Ltd.)

son's education he allowed him to be taken to school in the unusual clothes of his own design. James appeared in a tunic instead of a jacket, his square-toed shoes were fastened with brass clasps instead of black tape, and he had a frill instead of a round collar. After the first lesson he was surrounded by the other boys who pestered and teased him about his strange clothes. James replied to them ironically in broad Galloway dialect. His singular clothes and remarks prompted the boys to nickname him "Dafty." When he arrived home after school his tunic was in rags and his frill torn. He appeared to be amused and showed no irritation. His biographer rightly remarks that his behaviour probably concealed serious spiritual wounds.

Maxwell did not lose and did not try to get rid of his nickname during his school life. His peculiar remarks and laughter were treated as signs of idiocy.

For several years school did not touch his interest, which remained in "Glenlair" and his aunt's Edinburgh house. He wrote elaborate illustrated letters to his father who received them with affectionate understanding. When in Edinburgh his father never tired of showing him things. When he was twelve he was taken to see "electro-magnetic machines" and to a meeting of the Edinburgh Royal Society.

During this period his school-fellows continued to tease him. Sometimes he turned on them and fought them with demonic fury. His long friendship with Lewis Campbell appears to have begun in the school yard after Lewis had sided with him against tormentors.

At the age of thirteen he constructed models of "a tetrahedron, a dodecahedron and two other hedrons whose names I don't know" before he had begun the study of geometry at school.

He learnt the game of *diabolo* and developed great skill in it. *Diabolo* became a feature of "Glenlair" life and persisted into his Cambridge days. His interest in colour tops grew out of it.

With adolescence his intellectual development grew rapidly. He overcame his hesitancy in repeating lessons

by writing his words in the spaces of a plan of the large window in the rector's room and learning them in that setting. Next morning when called to repeat the lesson he would look at the actual window and see the words there through his imagination. He was fearful that a change of place in the class might prevent him from being in a position to look at the window.

From the beginning of his mathematical course Maxwell made swift progress. At the age of fourteen he won the mathematical medal, and wrote to his aunt that his friend Campbell had "got a letter written too soon congratulating him upon *my* medal; but there is no rivalry betwixt us." He also won a prize for English verse.

His father was keenly interested in his mathematical success and took him still more regularly to meetings of the Edinburgh Royal Society and Society of Arts. One of the lecturers to the Society of Arts was D. R. Hay, whose theories of the mathematical interpretation of beauty in form and colour had received much discussion. He spoke on the mathematical properties of the "egg-and-dart" patterns in Greek architectural ornaments. These raised the problem of how perfect ovals might be constructed. James studied the problem and discovered a method of drawing ovals with a pencil guided by a thread attached to pins. The value of economic independence for the encouragement of genius now became evident. Mr. Maxwell's freedom of occupation allowed him to secure immediate appreciation of his son's achievement as he devoted much time to visiting Hay and J. D. Forbes, the distinguished Edinburgh professor, and drawing their attention to the discovery. Forbes was much impressed by it and put Maxwell's argument into a form suitable for communication to the Royal Society of Edinburgh. So before he was fifteen years old Maxwell was taken by his father to a meeting of the Royal Society of Edinburgh to hear his first paper read. Professor Forbes remarked that Mr. Maxwell's method of drawing ovals was simpler and more general than Descartes', and that it had not been suspected that these curves, whose optical properties had been discussed

mathematically by Newton and Huyghens, were susceptible of so simple a practical construction.

Descartes, Newton, Huyghens! What names to appear in the discussion of a schoolboy's mathematical discovery!

Maxwell acquired the important friendship of Forbes. His researches at this time were not restricted to geometry. He became interested in the properties of jellies and gutta-percha, apparently through reading Forbes' recent papers on the theory of the movement of glaciers containing discussions of the effect of pressure on solid, liquid and viscous bodies.

Maxwell's original contributions to human knowledge retarded his progress at school. Owing to the diversion of time from regular studies he did not win the mathematical medal in 1846. In the next and last year he was again first in mathematics and also in English and almost first in Latin. Maxwell was never heard to have regretted his classical education. He repeatedly said in later years that he considered the discovery of an author's meaning without help, except from a grammar and dictionary, was one of the best methods of training the mind.

When he was fifteen he was often absent from school owing to delicate health. He had become interested in Newton's rings and the polarization of light, so he was taken by his uncle to see Nicol, the inventor of the polarizing prism. Later in the same year, when he was sixteen, he entered Edinburgh University and attended the courses there for three years. He worked under very little supervision. He diligently attended the classes on logic, metaphysics and morals, and acquired the knowledge of general ideas that made him an educated besides a scientific man. At the age of seventeen he made copious notes under the influence of the metaphysician Hamilton on the properties of matter, space, time, force and sensation. While the notes are without particular originality, they help to show the development of Maxwell's precision of scientific thought and how he succeeded in avoiding the identification of scientific concepts with reality which destroyed the value of the philosophical ideas of nearly all of his scientific

contemporaries. It saved him, too, from the exaggerated respect of philosophy by scientists ignorant of it. He immediately appreciated George Boole's treatise on *The Mathematical Analysis of Logic* which had recently been published and is now considered to contain the foundation of the modern science of mathematical logic. Though he was encouraged by the professors Forbes, Kelland and Hamilton, he had little to do with the students. His behaviour continued to be gently eccentric. He dressed very neatly, but would not wear starched clothes or gloves, and travelled third class on the railway because he preferred hard seats. He was often silent at table, or played with the finger-glasses in order to observe optical effects. Maxwell believed in training the senses, and considered dullness of sense-perception a bad sign in a man.

Forbes allowed him the privilege of using his private scientific apparatus. After Maxwell had met Nicol he extended his researches with polarized light. Fresnel and Brewster had explained that unannealed glass ought to show the phenomenon of double refraction. This is due to differences in elasticity in a transparent material along different directions. When a ray of light falls on such a material it is split into two rays, one of which follows the line of least elasticity and the other the greatest. The light vibrations in the rays are also sorted into two planes respectively at right angles to each other.

Hence the paths of the rays may be used to reveal the elasticities in various directions. In normally uniform materials such as annealed glass the elasticities are the same in all directions. If such materials are submitted to strains by unannealing or pressure the elasticities along various directions are changed. When he was seventeen Maxwell suggested that double-refraction might be used for tracing strains produced by pressure in transparent materials such as gelatine. In recent years Professor E. G. Coker has converted this technique into an important branch of engineering. Models of complicated engineering structures are made in gelatine and submitted to loads proportional to those that the structures will receive in practice.

The strains produced in the structures by the loads may be determined by an optical examination of the model. The two branches of the ray split by the strained material may interfere with each other, so that certain or all colours may be removed from the transmitted rays, which present a coloured pattern to the observer. Maxwell devised his own optical arrangement for showing these patterns and painted water-colour pictures of them. He sent his drawings to Nicol, who appreciated them, and in return presented him with a pair of his polarizing prisms. Maxwell remained very proud of this gift. Maxwell's experiments on the passage of light through strained materials led him to a mathematical investigation of the behaviour of elastic solids, upon which he communicated a long paper to the Royal Society of Edinburgh when he was nineteen. This paper contained mathematical investigations of the bending of beams and twisting of cylinders and other problems of high engineering importance, with confirmations of his results by experiments with polarized light and distorted jellies.

These achievements aroused considerable interest in Maxwell's future. Forbes visited his father and urged that he should be sent to Cambridge. After much deliberation his father decided to send him to Peterhouse, where Thomson had distinguished himself a few years before and his school-fellow Tait had already entered. Maxwell's admirers have often discussed whether he would have gained by entering Cambridge earlier and curtailing his desultory and socially isolated studies at Edinburgh. Some thought he would then have completed his systematic training earlier and provided himself more quickly with the technique for attacking profound problems. Others thought his Edinburgh period of independence helped to strengthen his intellectual originality. Maxwell's command of mathematics never became quite equal to his physical insight. If he had gone to Cambridge earlier this slight unbalance might have been remedied. But earlier Cambridge discipline might also have reduced his originality; so the problem remains insoluble. The results of Maxwell's actual education proved

in the end brilliant enough, so it is fair to assume his early intellectual independence was very valuable. His father deserves much honour for providing him with such independence combined with careful attention. The immediate appreciation of Maxwell by Forbes, Nicol, Kelland and others was due largely to his father's assiduity in bringing his work to their notice. If the boy had been given merely the independence of neglect his early work would probably never have been noticed.

Maxwell entered Peterhouse in October, 1850. In spite of a deep presentiment that Cambridge would help him he was not immediately happy. His odd manners and Galloway dialect at first hampered his social life, though less than before; and he was bored by the elementary first year work. At the end of his first term he left Peterhouse and entered Trinity College. His Peterhouse tutor, Mr. Porter, and others had advised him to leave Peterhouse as the small college had few fellowships and the competition for them was very severe. When Maxwell personally applied to Dr. Thomson, then tutor of Trinity College, for permission to migrate to the college he appeared shy and diffident, but presently surprised him by producing a bundle of copies of his original papers, remarking: "Perhaps these may show you that I am not unfit to enter at your College."

By the end of his first year he had made many friends. His playfulness and originality was much appreciated by the more intelligent undergraduates who could enjoy his obliquities. In 1852 he was awarded a college scholarship and henceforth dined at the scholar's table. He was particularly friendly with students of the classics. He sought to acquire more conventional manners, and the expression of his countenance changed markedly, becoming more serious and mature. The fascination of his personality became fully developed. He was happier, and returned to the composition of verse, which he had not practised for several years.

Presently he became a pupil of William Hopkins, the famous mathematical tutor who had prepared Stokes,

Thomson and many other distinguished candidates for the tripos examination. Considering the degree of his mental development and originality, he followed Hopkins' guidance with remarkable conscientiousness and played as well as he could the strange game of Wrangling. Hopkins was shocked by the disorder of Maxwell's immense store of knowledge, but he recognized his genius, for he said that Maxwell was by far the most remarkable of all the remarkable pupils he had had. He said he was almost incapable of thinking incorrectly on physical subjects though his command of formal mathematics was defective.

Maxwell was elected a member of an undergraduates' club familiarly named the *Apostles*, because their number was restricted to twelve. They considered themselves the ablest undergraduates in the university, and wrote essays, chiefly on philosophical themes, for mutual instruction. The extraordinary clarity of Maxwell's prose was cultivated by his writing for the club. He had a powerful visual imagination and could think extremely clearly, especially with the assistance of diagrams and models. When he was an undergraduate his preference for geometrical methods of solving problems was noticed. He did not use mathematics happily unless he could interpret the physical meaning of every step in the proof. His contemporaries were much impressed by this characteristic and assumed he had a pure engineering imagination. In fact, the most important characteristic of Maxwell's genius was his silent abandonment of this method of thought when he had stretched it to the limit of applicability.

In the summer of 1853, when he was reading hard and simultaneously enjoying a varied social life, he became seriously ill. His illness was described in the terminology of the day as "brain fever." He had to reduce the intensity of his application during the months preceding the examination. In this period of delicacy he became interested in F. D. Maurice's religious and social ideas, and was strongly attracted by his combination of Christian earnestness with universal sympathy.

The ablest of Maxwell's fellow-students felt that he had

a very uncommon personality, but did not appreciate the magnitude of his intellectual power. They regarded him as the most genial and amusing companion. The most serious students were shocked by the irregularity of his working habits. G. W. H. Tayler has described the expressiveness of Maxwell's countenance as he listened to lectures. He listened with entire concentration, and his features showed an acuity of intellect. Sometimes he smiled slightly as a point became clear to him or an amusing fancy flitted across his mind. Tayler records that he disliked the repetition of speculations from books. He preferred any original speculation and quaint original books that stimulated it, such as Sir Thomas Browne's *Religio Medici*.

In January, 1854, he sat for the examination. He told a friend that his mind was blank when he entered the hall for the first paper, but presently it became preternaturally clear. When he left he was dizzy and staggering. His old school-fellow Tait, who had been Senior Wrangler in the previous year, said that no good student had ever entered the examination worse prepared than Maxwell. But largely through a concentrated mental effort he gained second place in the competition.

The first place was won by E. J. Routh, a very capable and talented mathematician, whose facility, like Stephen Parkinson's, the senior to Thomson, had gained more marks than the genius of his competitor.

Maxwell's father was pleased by the result, writing that he had gained a higher place than was generally expected, and that he would call on his old mathematics teacher and congratulate him on his pupil's success.

He did not publish any original researches in the four years separating his departure from Edinburgh University and his graduation at Cambridge. Thomson published a dozen papers, including several of high importance, in the parallel period. Considering the brilliance of the start of Maxwell's researches at the age of fourteen, the output during the succeeding nine years was small. At twenty-three he had published only four papers. Some have

regretted that he gave so much time to examination preparation and general cultivation, but this conduct was probably wise. The most serious deficiency in his youth was lack of adaptation to social intercourse. The history of physics shows that the reform of Cambridge teaching in physical science was one of his greatest achievements. He could not have accomplished this if he had been socially isolated and unassimilated by the society he sought to reform. During his four undergraduate years he secured the confidence of many able men at Cambridge, especially non-scientific students. His proposals of reform in later years received the support of many who liked him, irrespective of the detail of his arguments. While a better examination system might have allowed him to continue original research besides developing himself socially, it is permissible in an imperfect world to be quite satisfied with his ultimate accomplishment of all his major objects.

Soon after he had graduated he wrote to Thomson asking for advice concerning research. He said he had thought of studying electricity, and inquired whether Thomson would mind if he entered the same field. He evidently received an encouraging reply as he wrote to his father that Thomson "is very glad that I should poach on his electrical preserves."

While he was preparing the material for his first great paper, *On Faraday's Lines of Force*, he was intellectually very active in various directions. He had never stopped the investigations of colour-sensation suggested, like his work on the geometry of oval curves, by D. R. Hay's book on the mathematical theory of art. He published several papers on this subject early in 1855, some account of which will presently be given.

He continued to write essays for his club, and started, under Maurice's influence, to give lectures to working men.

The extant essays discuss subjects such as *Decision: What is the nature of evidence of design? The dark sciences: Has everything beautiful in art its original in nature? Is the modern vocabulary of the English language the effect or the cause of its speculative state? Are there real analogies in*

nature? Is ethical truth obtainable from an individual point of view? Is autobiography possible? Is a horror of unnecessary thought natural or unnatural?

The style of these essays may be indicated by a few quotations from them. Of indecision he writes when it "cannot be traced to hypochondria, we generally find indications of a defective appreciation of quantity and a deceptive memory." The essentials of true evidence of design are "(1) a phenomenon having significance to us; (2) two ascertained chains of physical causes contingently connected, and both having the same apparent terminations, viz. the phenomenon itself and some presupposed personality." "Every well-ascertained law points to some central cause, and at once constitutes that centre a *being* in the general sense of the word. Whether that being be *personal* is a question which may be determined by induction." Maxwell evidently agreed with Newton that the existence of a mechanism operating according to one principle implied the existence of a principal behind the principle. He says the search for invisible potencies or wisdoms, centres of causal chains, may appear novel and unsanctioned. "For my part I do not think that any speculations about the personality or intelligence of subordinate agents in creation could ever be perverted into witchcraft or demonolatry."

In the development of the Dark Sciences he notices three phases. In the first they imitated popular physics and professed to explain occult phenomena by means of new and still more occult material laws. Experimenters in animal magnetism always performed with their noses to the north and electro-biologists practised a scrupulous system of insulation. In the second phase the phraseology of physics is exchanged for that of psychology. "The verb to will acquires a new and popular sense, so that everyone now is able to will a thing without bequeathing it. People can will not to be able to do a thing, then try and not succeed, while those of stronger minds can will their victims out of their wits and back again."

The third or pneumatological phase begins by distrusting

the previous methods of explanation. "It suggests that different minds may have some communion, though separated by space, through some spiritual medium. Such a suggestion if discreetly followed up might lead to important discoveries and would certainly give rise to entertaining meditations. But the cultivators of the dark sciences have done as they have ever delighted to do. Their spirits . . . become the familiar spirits of money-making media." . . . "The powerful analysis of Godfrey has led him to the conclusion that a table of which the plane surface is touched by believing fingers may be transformed into a diaboloid of revolution."

In the essay on Analogies he remarks: "There is nothing more essential to the right understanding of things than a perception of the relations of *number*. Now the very first notion of number implies a previous act of intelligence. Before we can count any number of things we must pick them out of the universe . . . until we have done this the universe of sense is neither one nor many, but indefinite . . . the dimmed outlines of phenomenal things all merge into another unless we put on the focussing glass of theory . . . as for space and time, any man will tell you that 'it is now known and ascertained that they are merely modifications of our own minds' . . . but 'if we admit that we can think of difference independent of sequence, and of sequence without difference, we have admitted enough on which to found the possibility of the ideas of space and time.' . . . Perhaps the 'book,' as it has been called, of nature is regularly paged; if so, no doubt the introductory parts will explain those that follow, and the methods taught in the first chapters will be taken for granted and used as illustrations in the more advanced parts of the course; but if it is not a 'book' at all, but a *magazine*, nothing is more foolish to suppose than that one part can throw light on another" . . . "the only laws of matter are those which our minds must fabricate, and the only laws of mind are fabricated for it by matter."

Maxwell was twenty-four when he expounded these views. It is not surprising that his scientific researches

afterwards showed long marches towards the theories of relativity and the quantum.

The quality of his early verse is illustrated by his stanzas on *Reflection from Various Surfaces*, composed when he was twenty-one:

In the dense entangled street,
Where the web of Trade is weaving,
Forms unknown in crowds I meet
Much of each and all believing;
Each his small designs achieving
Hurries on with restless feet,
While, through Fancy's power deceiving,
Self in every form I greet.

Oft in yonder rocky dell
Neath the birches' shadow seated
I have watched the darksome well,
Where my stooping form, repeated,
Now advanced and now retreated
With the springs alternate swell,
Till destroyed before completed
As the big drops grew and fell.

By the hollow mountain-side
Questions strange I shout for ever,
While the echoes far and wide
Seem to mock my vain endeavour;
Still I shout, for though they never
Cast my borrowed voice aside,
Words from empty words they sever—
Words of Truth from words of Pride.

Yes, the faces in the crowd,
And the wakened echoes, glancing,
From the mountain, rocky browed,
And the lights in water dancing—
Each, my wandering sense entrancing,
Tells me back my thoughts aloud,
All the joys of Truth enhancing
Crushing all that makes me proud.

Maxwell's first paper on colour vision was published in 1855. It was a product of the stimulus given to him when he was fourteen by D. R. Hay's scientific theories of art. He describes a "method by which every variety of visible

JAMES CLERK MAXWELL AS A YOUNG MAN

(The Master and Fellows of Trinity College, Cambridge

colour may be exhibited to the eye in such a form as to admit of accurate comparison; to show how experiments so made may be registered numerically; and to deduce from these numerical results certain laws of vision." He produced various tints "by means of a combination of discs of paper, painted with the pigments commonly used in the arts and arranged round an axis, so that a sector of any required angular magnitude of each colour may be exposed. When this system of discs is set in rapid rotation the sectors of the different colours become indistinguishable and the whole appears of one uniform tint."

With this apparatus Maxwell compared the equivalence of different combinations and quantities of colours. For instance, a combination of a carmine sector of size ·44, with an ultramarine sector of size ·22, and an emerald green of ·34 gave the same tint when spun in sunlight as a snow-white sector of size ·17 with an ivory black of ·83. Under gas light the respective sizes of the sectors were given by the equation:

·47 C.-

5 S.W.+·75

"which shows that the yellowing effect of the gaslight tells more on the white than on the combination of colours."

He concluded from a number of experiments with different observers that the human eye is capable of very precise estimation of the likeness of colours, that the judgment is determined by a cause residing in the eye of the observer and not by the real identity of the colours, and that the law of colour-vision is, within a certain degree of accuracy, identical for all ordinary eyes.

Maxwell showed nearly every colour can be matched by combinations of three other colours, so these may be accepted as the primary colours. He adopted as the primary colours certain wave-lengths in the red, green and violet parts of the spectrum. He found colour-blind persons can match any of their colour-sensations with combinations of two primary colours, confirming the theory that colour-blindness is due to a defect in one of the three primary sensations upon which perception of colour depends.

The physical analysis of colour-sensation was conducted still more elegantly by Helmholtz, who in 1852 had introduced the method of mixing pure colours selected optically from the spectrum. The reflected light from coloured discs did not give pure colours and varied according to the nature of the incident light.

Maxwell also employed the mixture of pure spectral colours in his colour box after, but independently, of Helmholtz.

Increase in scientific reputation was the most important result of Maxwell's researches on colour-vision. Historically, the physics of colour was highly esteemed in Britain because it had been founded by Newton and Young and studied in Maxwell's youth by Brewster and Forbes. The subject was fashionable and contributions received immediate appreciation. Maxwell's name was added to those of Newton and Young as a contributor of fundamental knowledge to the theory of colour-vision; and in 1860 the Royal Society presented him with the Rumford Medal for these researches.

Maxwell described the results of his new study of electricity begun in 1854 in his paper, *On Faraday's Lines of Force*, read in 1855 and 1856. In this paper another aspect of his genius appears, in addition to the variety of aspects already displayed in his earlier researches. In addition to remarkable independence, clarity and subtlety, he begins to show a systematic deliberation. He examines the philosophical state of electrical theory and then one by one the contemporary conceptions of the chief electrical and magnetic phenomena. His possession of high intellectual tenacity was not obviously suggested by his previous career. All men of judgment were aware of his intellectual brilliance, but not all recognized, even by the date of his death, his formidable thoroughness. Perhaps he acquired some of this quality from his study of Faraday's *Experimental Researches*. The key of the intellectual music of his paper *On Faraday's Lines of Force* is in tune with the *Experimental Researches* and is different from that of his earlier papers on other themes.

In the first sentence of his writings on electricity Maxwell says: "The present state of electrical science seems peculiarly unfavourable to speculation." He remarks that some of the phenomena of statical electricity, current electricity and electro-magnetism can be described mathematically, but no general theory connecting these types of phenomena together had as yet been found. The student searching for a general theory must be familiar with a "considerable body of most intricate mathematics, the mere retention of which in the memory materially interferes with further progress." If the unification of the different branches of electrical theory is to proceed some method of simplifying the systems of ideas in the different branches must be found so that the student can bring the chief concepts of each simultaneously before his mind. The simplification may be effected in two ways: by finding a common denominator in the shape of a mathematical formula or a physical hypothesis. "In the first case we entirely lose sight of the phenomena to be explained; and though we may trace out the consequences of given laws we can never obtain more extended views of the connexions of the subject. If, on the other hand, we adopt a physical hypothesis we see the phenomena only through a medium and are liable to that blindness to facts and rashness in assumption which a partial explanation encourages. We must therefore discover some method of investigation which allows the mind at every step to lay hold of a clear physical conception without being committed to any theory founded on the physical science from which that conception is borrowed, so that it is neither drawn aside from the subject in the pursuit of analytical subtleties nor carried beyond the truth by a favourite hypothesis.

In order to obtain physical ideas without adopting a physical theory we must make ourselves familiar with the existence of physical analogies. By a physical analogy I mean that partial similarity between the laws of one science and those of another which makes each of them illustrate the other. Thus all the mathematical sciences are founded on relations between physical laws and laws of numbers, so that the aim of exact science is to reduce the problems of

nature to the determination of quantities by operations with numbers."

He quotes the analogy of a ray of light with the path of a particle and of a wave. Then he comments on the analogy of the formulæ describing the flow of heat with those relating to attractions demonstrated by Thomson when a youth. In the discussion of the conduction of heat "the word *force* is foreign to the subject. Yet we find that the mathematical laws of the uniform motion of heat in homogeneous media are identical in form with those of attractions varying inversely as the square of the distance."

"Now the conduction of heat is supposed to proceed by an action between contiguous parts of a medium, while the force of attraction is a relation between distant bodies, and yet if we knew nothing more than is expressed in the mathematical formulæ there would be nothing to distinguish between the one set of phenomena and the other."

He explains that "lines of force" may conveniently be represented by "fine tubes of variable section carrying an incompressible fluid." The intensity besides the direction of the force at any point may be represented by the movement of the fluid. "In the case of a perfectly arbitrary system of forces there will generally be interstices between the tubes, but in the case of electric and magnetic forces it is possible to arrange the tubes so as to leave no interstices. The tubes will then be mere surfaces, directing the motion of a fluid filling up all space." He remarks that the mathematical study of electric and magnetic forces has usually been based on the description of a model in which these forces are assumed to be analogous to reactions between certain points, but he now proposes to conduct the mathematical study on the assumption that the reactions of the forces are analogous to the reactions in the hydrodynamical model he has just described. He then proceeds to show that "the laws of the attractions and inductive actions of magnets and currents may be clearly conceived, without making any assumptions as to the physical nature of electricity, or adding anything to that which has been already proved by experiment." He has already written

that he is "not attempting to establish any physical theory of a science in which I have hardly made a single experiment."

He discusses Faraday's notion of the electro-tonic state, and regrets he has been unable to formulate a conception of it without the use of mathematical symbolism. "We may conceive of the electro-tonic state at any point of space as a quantity determinate in magnitude and direction, and we may represent the electro-tonic condition of a portion of space by any mechanical system which has at every point some quantity, which may be a velocity, a displacement, or a force."

Maxwell's hydrodynamical model had presented the transmission of electrical force as analogous to the movement of an incompressible fluid through space. As there were no interstices in this fluid the transmission must operate through the movement of surfaces in the fluid. Maxwell had not yet begun to see this moving surface as a wave-front, so he had as yet only begun the journey towards the wave-theory of light.

The failure of everyone except Maxwell himself to extend the researches in this wonderful first paper on electricity must attract the student of the evolution of scientific ideas, and of the history of science. Sir J. J. Thomson writes that the older men were not prevented by hidebound conservatism or wilful blindness to evidence from following Maxwell's lead. They had been reared in theories of action-at-a-distance, that had had great successes in describing a variety of electric and magnetic phenomena. Besides this success in electrical theory, the concept of action-at-a-distance had behind it all of the prestige of the triumphs of gravitational dynamics. No experimental electrical facts in conflict with the notion of action-at-a distance were known.

The eminent Astronomer Royal, Sir George Biddell Airy, declared that he could "hardly imagine any one who knows the agreement between observation and calculation based on action at a distance to hesitate an instant between this simple and precise action on the one hand and anything so vague and varying as lines of force on the other."

Owing to his understanding of the spirit of scientific experimentation and his geometrical imagination, Maxwell was convinced of the correctness of Faraday's conceptions, without making any experimental electrical investigations himself. Faraday's appreciation of Maxwell's work was immediate. In a letter dated November 13th, 1857, he wrote: "I have always found that you could convey to me a perfectly clear idea of your conclusions, which, though they may give me no full understanding of the steps of your process, give me the results neither above nor below the truth, and so clear in character that I can think and work from them."

But how was he to secure the confidence of the Airys who found the complications of lines of force absurd in comparison with the simplicity of action-at-a-distance? Maxwell executed, unconsciously, one of the most dramatic actions in the history of scientific ideas. He undertook one of the most difficult problems in gravitational dynamics: the investigation of the nature of Saturn's rings, and proved they must consist of revolving clouds of tiny satellites. Airy himself said this was one of the most remarkable applications of mathematics to physics he had ever seen.

Maxwell had demonstrated with unquestionable brilliance his command of action-at-a-distance conceptions, and yet he advocated the adoption of Faraday's apparently complicated ideas of lines-of-force. What could be the matter with the man? His contemporaries were puzzled, and assumed it was just one more of his vagaries.

His memoir on Saturn's rings greatly increased his reputation. In this way, it proved to be of high tactical importance in the semiconscious campaign for the capture and reformation of Cambridge science, as it prevented the older critics from questioning Maxwell's proficiency in classical physics. In his investigations of colour-physics and Saturn's rings Maxwell had qualified by taste and manner in the school of the most conventional followers of Newton. He used the position he had gained by this conduct for the introduction of post-Newtonian ideas. By his actions Maxwell showed he was one of the most superb

politicians of ideas known in the history of culture. That Maxwell introduced Faraday's ideas into theoretical physics, is a commonplace. Not enough attention has been given to the extraordinary nature of the sequence of achievements, and combination of intellectual qualities, by which he gained intellectual power through one policy and used it to introduce another.

Maxwell was now twenty-four. He had very fully and actively settled as a fellow of Trinity College. The course of his life would seem to have been laid for many years. But his absorption in Cambridge scientific life was prevented by his father's declining health, and also a perception of the limitations of college life. That he should have perceived these at once is yet another example of the varied insight of his genius.

While he was preparing the first paper on Faraday's lines of force, he was lecturing at the Cambridge Working Men's classes on decimal fractions. In March, 1856, he wrote to his father: "We are getting up a preparatory school for biggish boys to get up their preliminaries. We are also agitating in favour of early closing of shops. We have got the whole of the ironmongers, and all the shoemakers, but one. The booksellers have done it some time. The Pitt Press keeps late hours, and is to be petitioned to shut up. I have just written out an abstract of the second part of my paper on Faraday's Lines of Force. . . ."

J. D. Forbes had written a few days before, informing him the chair of natural philosophy at Marischal College, Aberdeen, was vacant. Maxwell decided to apply for it, in order to be nearer his sick father, and also, as he wrote: "I think the sooner I get into regular work the better, and that the best way of getting into such work is to profess one's readiness by applying it. . . . I suppose the correct thing to do is to send certificates of merit, signed by swells, to one or other of these officers. . . . In all ordinary affairs political distinctions are supposed to weigh a great deal in Scotland. The English notion is that in pure and even in mixed mathematics politics are of little use, however much a knowledge of these sciences may promote the study of

politics. As to theology, I am not aware that the mathematicians, as a body, are guilty of any heresies. . . ."

"If you believe the testimonials you would think the Government had in their hands the triumph or downfall of education generally, according as they elected one or not. . . ."

His father replied: "I believe there is some salary, but fees and pupils, I think, cannot be very plenty. But if the *postie* be gotten, and prove not good, it can be given up; at any rate it occupies but half the year."

The difference between the attitudes of the Maxwell family and the Thomson family in their pursuit of chairs is arresting. Thomson's father, as a professional man risen from the class of small farmers, passionately desired position and salary for his son. Maxwell's father, as a small landowner, was detached in his judgment, and considered a merit of the chair was that "at any rate it occupies but half the year."

His son referred to possible writers of testimonials as "swells." The Thomsons would have considered as sacrilegious the application of such terminology to persons influencing the acquisition of chairs.

On April 2nd his father died, but the application for the chair proceeded, and he was elected later in the month. He wrote to his friend Litchfield: "It is not pleasant to go down to live solitary but it would not be pleasant to stay up either, when all one had to do lay elsewhere. The transition state from a man into a Don must come at last, and it must be painful, like gradual outrooting of nerves. When it is done there is no more pain but occasional reminders from some suckers, tap-roots, or other remnants of the old nerves, just to show what was there and what might have been."

Maxwell undertook his Aberdeen duties earnestly, but he remained there for only three years. In 1860 Marischal College and King's College were amalgamated, and his chair was suppressed. The occupant of the King's College chair was David Thomson, a nephew of Faraday and a very capable teacher. He was senior to Maxwell, and he

naturally held the precedence. During his Aberdeen period Maxwell finished his investigation of Saturn's rings. The object of the investigation was to "explain how a material system, presenting the appearance of Saturn's rings, can be maintained in permanent motion consistently with the laws of gravitation." It was supposed the rings might be composed of solid, fluid or loose materials. He concluded from his analysis that a uniform solid ring could not be stable. A ring loaded so that 82 per cent of its total mass were on one side and 18 per cent on the other, would be stable, but only if the loading were exactly adjusted between the limits of 81 per cent and 83 per cent. A ring loaded outside these limits would be destroyed by collision between its inside edge and the surface of the planet. As observation shows the existence of such extreme and peculiar loading is improbable, the rings must be fluid or particulate. If the rings were fluid, waves would arise in it and break it into portions, the number of which would depend on the mass of Saturn directly and on that of the ring inversely. Hence the rings must consist of disconnected masses, which, it appears, may be fluid or solid and need not be equal. The internal motions of such rings were proved to be governed by four systems of waves, which could be combined to describe all the data of observation. Maxwell designed a mechanical model of one of the wave-motions. His conclusion that the rings were particulate was confirmed by the observation that the rays of light from the surface of the planet were not refracted by the material of the recently discovered obscure inner ring. Thirty-eight years later Keeler proved spectroscopically that the particles in the inner rings were revolving more rapidly than those in the outer rings. The memoir was submitted for the Adams Prize, for which Thomson was one of the examiners. Maxwell wrote "great screeds" to him about "those Rings, and lo! he was a-laying of the telegraph which was to go to America." In the train to Glasgow he composed "The Song of the Atlantic Telegraph Company." He defines (U) = "Under the sea," and 2(U) "represents two repetitions of that sentiment."

2(U)
Mark how the telegraph motions to me,
2(U)
Signals are coming along,
With a wag, wag, wag;
The telegraph needle is vibrating free,
And every vibration is telling to me
How they drag, drag, drag,
The telegraph cable along.

No little signals are coming to me,
2(U)
Something has surely gone wrong,
And it's broke, broke, broke;
What is the cause of it does not transpire,
But something has broken the telegraph wire
With a stroke, stroke, stroke,
Or else they've been pulling too strong.

III

2(U)
Fishes are whispering. What can it be,
2(U)
So many hundred miles long?
For it's strange, strange, strange,
How they could spin out such durable stuff,
Lying all wiry, elastic, and tough,
Without change, change, change,
In the salt water so strong.

IV

2(U)
There let us leave it for fishes to see;
2(U)
They'll see lots of cables ere long,
For we'll twine, twine, twine,
And spin a new cable, and try it again,
And settle our bargains of cotton and grain,
With a line, line, line,—
A line that will never go wrong.

Maxwell's examination of the properties of the "flight of brickbats," as he described Saturn's rings, is said to have been the origin of his researches into the dynamical theory of gases. His first important contributions to this theory were read at the Aberdeen meeting of the British Association in 1859.

Maxwell reported soon after he had gone to Aberdeen that "No jokes of any kind are understood here, I have not made one for two months and if I feel one coming I shall bite my tongue." Nevertheless, he married the daughter of the Principal of Marischal College.

He was not much upset by his departure from Aberdeen. He was quite happy acting as the laird of "Glenlair," and continuing the life his father had organized. The chair of natural philosophy at Edinburgh became vacant through the retirement of Forbes, and Maxwell applied for it, but P. G. Tait was preferred by the electors. The Edinburgh *Courant* contained the comment that "Professor Maxwell is already acknowledged to be one of the most remarkable men known to the scientific world . . . there is another quality which is desirable in a Professor in a University like ours and that is the power of oral exposition proceeding on the supposition of imperfect knowledge or even total ignorance on the part of pupils. We have little doubt that it was a deficiency in this power in Professor Maxwell which made the Curators prefer the claims of Mr. Tait." As Sir J. J. Thomson writes, the view that Maxwell was unsuccessful as a teacher must seem strange to those who know him only through his writings, but it is possible he drifted away from the level of his students' understanding, when he did not read his lectures from manuscript.

At any rate, two universities in his native land had the opportunity of retaining him as their professor, but both preferred teachers. In the condition of Scottish education to-day some critics see the retribution for the preference of teaching ability to creative genius. In the long run, this policy does not pay.

After two repulses one might have expected Maxwell to have retreated from the pursuit of a professional career.

But nineteenth-century culture had more fortune than it deserved. Later in 1860 the chair at King's College, London, became vacant, and Maxwell was appointed. He occupied this chair for five extremely productive years, until he retired owing to ill-health. In the autumn of 1860 he had had an attack of smallpox at "Glenlair," and in 1865 he was scratched on the head by a bough while riding. This was followed by a severe attack of erysipelas.

The delivery of lectures to working men was one of his duties at King's College. He continued to deliver these lectures during the year following his resignation of the chair.

Though Maxwell's researches on the kinetic theory of gases is said to have been inspired by his investigation of Saturn's rings, passages in his own writings seem to suggest his interest was aroused by Clausius' work.

The conception of gases as collections of flying particles was one of the earliest achievements of the atomic theory attributed to Democritus. The mathematical statement of this conception was not begun until 1738, when Daniel Bernouilli proved that the pressure of a gas, if it arose from the impact of moving particles, must be proportional to the square of their velocity. The next advance appears to be due to Herapath about 1816, when he obtained a formula which indicated that the product of the pressure and volume of a gas should be equal to one-third of the square of the velocity of the molecules. Herapath assumed the temperature of the gas was proportional, not to the square of the velocity, but directly to the velocity. This was erroneous, but did not invalidate his deduction of Boyle's law from the kinetic theory. According to his assumption, the velocity remained constant at constant temperature, so the product of pressure and volume remained constant. The nature of the mathematical relation between the velocity of the molecules and temperature was irrelevant to this part of the argument. In 1846 J. J. Waterston read a paper to the Royal Society in which he explained that the velocities of all the molecules were not equal. Their mutual collisions must produce a variety of velocities, and

the pressure of a gas must be equal to one-third of the product of the number of mass, and the mean square of the velocities of all the molecules. Waterston correctly assumed temperature to be proportional to the square of the velocity. He deduced that if collisions of molecules made them spin, the energy of rotation has a definite proportion to the energy of translation. He showed that the product of pressure and volume must be equal to two-thirds of the product of the number of molecules, and the absolute temperature; and even arrived at the result that "in mixed media the mean square molecular velocity is inversely proportional to the specific weights of the molecules." The Royal Society was unable to appreciate these great discoveries, and put his paper into their archives, in which it was found by Rayleigh in 1892. Waterston lived in Bombay. Nevertheless, he brought his researches to the notice of the British Association in 1851, and published an account of them in the *Philosophical Magazine* in 1858, but they remained unnoticed. Unlike Joule, he found no Thomson to support him. The failure to appreciate Waterston's researches is an instructive episode in the history of physics in the nineteenth century. It illustrates the condition of the Royal Society in 1846, and the remarkable difficulty with which the human mind adopted the concepts necessary for the comprehension of the principles of the steam-engine. The history of the discoveries of Carnot and Joule has parallel features. The leaders of physics during the first half of the nineteenth century did not appreciate thermodynamics and the kinetic theory of gases, because they were still engaged on the scientific problems of an order of society preceding the industrial. Waterston lived in Bombay, and did not move in those London social strata whose opinions at that time ruled scientific thought. The scientific societies had not yet been captured by the industrialists, and Waterston did not belong to the social class that ruled them, so his ideas and his social connexions were foreign to the contemporary governors of science. Thomson and Maxwell, the leaders of the physical scientific part of the culture of industrialism,

had respectively to establish the respectability of the ideas of Carnot and Joule, and to rediscover the ideas of Waterston. They were in sympathy with the new cultural development, but held positions in the universities that could be socially effective, so they had the power, that Carnot, Joule and Waterston had not, to secure the reception of their ideas.

In 1848 Joule calculated by Herapath's formula the mean velocity of a molecule of hydrogen at atmospheric pressure and the freezing point of water, obtaining the value of 6,055 feet per second. Common experience shows, however, that molecules of gases are not capable of travelling continuously at that order of speed through air. If a bottle of ammonia is opened in a room, several seconds may elapse before the odour is noticeable at a distance of a few yards. Clausius explained this phenomenon by pointing out that the molecules travel at the speed corresponding to their temperature only when they are moving freely. They are continually being stopped by collisions which may change their direction. The rate of diffusion of gases depends on the distance their molecules travel between each collision, besides the speed of movement. Hence Clausius devised the conception of the "mean free path," as a mathematically manageable measure of the average distance travelled by a molecule between two collisions. In 1859 he published a calculation of the length of the "mean free path" in terms of the average distance between the molecules in a volume of gas, and the distance between the centres of two colliding molecules at the moment of impact. Maxwell read Clausius' papers, and immediately devoted his full power to the development of the dynamical theory of gases. He spoke on the theory at the British Association meeting at Aberdeen in 1859. Clausius and his predecessors, except the unnoticed Waterston, assumed all the molecules were moving at the same speed. This clearly could not occur in fact, because the collisions would sometimes increase, and sometimes decrease the speed of a colliding molecule. If the speeds had all been the same at any moment they would very swiftly come to be varied by the collisions in the next moments. The dynamical theory of

gases could not be developed further without the discovery of a mathematical method of deducing the actual velocity of any molecule selected at random.

In his Aberdeen lecture Maxwell gave a solution of this problem with the assistance of the mathematical theory of probability. He showed the speeds were distributed among the molecules according to the same law as the errors in a group of observations. They varied from zero to infinity, but the number of very swift molecules was comparatively small. The sequence of Maxwell's original analysis of the distribution of molecular speeds is not at all clear, though the result is correct. Jeans has written that his argument "seems to bear no relation at all to molecules, or to the dynamics of their movements, or to logic, or even to ordinary common sense," but gave "a formula which, according to all precedents and all the rules of scientific philosophy, ought to have been hopelessly wrong. In actual fact, it was subsequently shown to be exactly right and is known as Maxwell's law to this day." In his determination of the molecular velocities, he had invented the science of statistical mechanics. It is said the idea of applying statistics to the movements, or mechanism of matter, was suggested to him by his investigation of Saturn's rings. After Maxwell had revealed the statistical aspect of the mechanism of matter, the description of natural phenomena became increasingly based on the theory of probability. Intimations of the quantum theory of action appeared as early as 1877 in the researches of Boltzmann, and came into being with Planck in 1900. The theory of probability has now penetrated through the whole of natural phenomena, and classical mechanics appears only as a simplified form of the more fundamental statistical mechanics.

Maxwell completed his first paper by calculating the velocity, mean free path and number of collisions of the molecules in the air at 60° F. The velocity of the molecules in air was 1,505 feet per second, the mean free path was $\frac{1}{447,000}$ of an inch, each molecule made 8,077,200,000 collisions per second.

He also explained the viscosity of a gas should be independent of its density. This was in remarkable conflict with the common-sense view that when the density of a gas was high it would impede the passage of an object more than when its density was low, because the molecules would strike and impede the object more frequently when the density was high.

Maxwell showed that the degree of retardation by friction with the molecules depended on the length of the mean free path. As the mean free path increases with the decrease of density, the net effect of decreasing the density of the gas is to leave the degree of retardation constant. Maxwell subsequently proved by experiment that the viscosity of air at a pressure of 0·5 inches was the same as at 30 inches. The experiments were made in an attic of his house in London. Mrs. Maxwell acted as a stoker and regulator of temperature.

Maxwell gave the first expression of the pressure of a gas based on the supposition of random molecular speeds, and showed it was the same as that obtained by his predecessors on the assumption of uniform speeds. He repeated, more correctly, Waterston's deduction of Avogadro's law, that equal volumes of gas, at equal temperatures and pressures, contain equal numbers of molecules.

Planck has written that the importance of Maxwell's invention of statistical mechanics was immediately appreciated by Boltzmann, but not by Clausius. Maxwell had found that the ratio of the specific heats of a gas at constant pressure and volume could be deduced to be $1\frac{2}{3}$, if the molecules were spherical. Experiment showed that the ratio was correct for mercury gas, whose molecules consist of single atoms. But the calculated and the observed ratios for hydrogen, oxygen and nitrogren, $1\frac{1}{3}$ and $1\frac{2}{5}$ respectively, do not agree, assuming these molecules have three moments of inertia. Boltzmann showed the calculated ratio would be correct on the reasonable assumption that the molecules of these diatomic gases have only two instead of three moments of inertia.

In spite of all these triumphs of the new dynamical

theory, Maxwell and Boltzmann found the exact deduction of the laws of viscosity, diffusion and conduction of heat in gases very difficult. Boltzmann succeeded in finding an equation for the distribution of molecular speeds in a gas not in a steady state, but he could not solve it in the case when the molecules were elastic spheres, though he made most laborious and lengthy attempts.

Maxwell finally evaded the difficulty by inventing a sort of molecule for which the equation could be solved, and which was sufficiently like actual molecules to provide a tenable description of them. He conceived the molecule as a centre of force. By choosing a suitable law of interaction between two centres of force or molecules, all of the phenomena of elastic collision could be satisfactorily represented. If the centres repelled each other in inverse proportion to a fairly high power of their mutual distance, they would move almost independently at great distances, but separate under great repulsive forces when close together. Maxwell noticed that if the force was assumed to vary inversely as the fifth power of the distance, the relative velocity of the molecules disappears from the expression for the viscosity, and the viscosity of the gas can be deduced with comparative ease without requiring a general expression for the distribution of molecular speeds in a gas not in a steady state.

No one could appreciate this achievement better than Boltzmann, who considered it a perfect work of art, and compared Maxwell's memoir to a musical drama. He wrote: "A mathematician will recognize Cauchy, Gauss, Jacobi, Helmholtz, after reading a few pages, just as musicians recognize, from the first few bars, Mozart, Beethoven or Schubert. Perfect elegance of expression belongs to the French, though it is occasionally combined with some weakness in the construction of the conclusions; the greatest dramatic vigour to the English, and above all to Maxwell. Who does not know his dynamical theory of gases? At first the Variations of the Velocities are developed majestically, then from one side enter the Equations of State, from the other the Equations of Motion in a Central

Field; ever higher sweeps the chaos of Formulæ; suddenly are heard the four words: 'put $n=5$.' The evil spirit V (the relative velocity of two molecules) vanishes and the dominating figure in the bass is suddenly silent; that which had seemed insuperable being overcome as by a magic stroke. There is no time to say why this or why that substitution was made; who cannot sense this should lay the book aside, for Maxwell is no writer of programme music who is obliged to set the explanation over the score. Result after result is given by the pliant formulæ till, as unexpected climax, comes the Heat Equilibrium of a heavy gas; the curtain then drops."

"I remember still how Kirchhoff, discussing this memoir with me, made the remark: 'This is the way to deal with gas theories.' As Schuster has remarked, this passage must not be taken too literally. Maxwell did not say 'put $n=5$' but 'It will be shown that we have reason from experiments on the viscosity of gases to believe that $n=5$.'" Jeans considers that Maxwell's researches into the dynamical theory of gases are the most brilliant manifestation of his genius. In these his power of obtaining profound results by provisional arguments is seen in its highest degree. Maxwell's scientific divination seems to be connected with an extraordinary gift of intellectual freedom. He could examine a problem without conceptual, logical or mathematical prejudice. Unlike so many voyagers to the frontier of knowledge, he did not stiffen his imagination in the struggle for new ideas. The mental effort necessary to form a new idea is large, and often strains the mind that forms the idea into a permanent mould. Maxwell seemed to be able to dissolve all these moulds as soon as they had served. The absence of prejudice, or obvious leading ideas, seemed to make his physics, in the words of Jeans, an enchanted fairyland where no one knew what was coming next. His apparently magical powers arose from his ability to examine each problem in isolation, and avoid the assumption that the methods evolved to solve one problem would necessarily serve for the solution of the next problem. This saved him from the error of applying methods suitable

for the solution of one problem to the solution of other problems for which they were unsuitable. His avoidance of this very common and natural mistake often made his methods seem almost intuitional, and intellectually unfair. But Maxwell's mind was not specially mysterious. His originality did not spring from some peculiar intuition, but from the magnitude of his intelligence. He could arrive at the frontiers of knowledge with less labour than others, and so, having arrived there with a larger surplus of mental energy, and a smaller burden of unsuitable technical equipment, he could set about him with greater freshness and without the encumbrance of unsuitable tools. Some rational explanation of Maxwell's apparently magical or intuitional powers might be found in this way. They arose from, and are a measure of, the strength of his intelligence.

While many keen judges find the most brilliant exhibitions of Maxwell's genius in his contributions to the dynamical theory of gases, the majority of physicists consider his discovery of the electro-magnetic theory of light his greatest achievement. The most brilliant work on both of these theories was done during Maxwell's tenure of the chair at King's College, London, between 1860 and 1865, and the ages of twenty-nine and thirty-four. His life at this period was very strenuous. He lectured during nine months of the year; a very long session in university work. He gave lectures to artizans as part of his duties. Besides his theoretical investigations of the dynamics of gases, and of electricity, he supervised the experimental determination of electrical units for the British Association's committee. The results became the basis for the system of electrical units accepted by international agreement. Maxwell's part in this work led him to interest his pupils in it. Glazebrook continued the work on units and became secretary of the British Association's committee. The determination of electrical units grew into the determination of physical units in general, and ultimately a special institution, the National Physical Laboratory, was founded to pursue the now extensive researches in measurement and standardization. During this period Maxwell measured the ratio of

the electro-magnetic and the electrostatic units of electricity. According to his electro-magnetic theory of light, this ratio should be equal to the velocity of light. His method consisted in comparing the attractions between two currents flowing through wire coils, and the attraction or repulsion between two electrically charged metal plates, and his result was in satisfactory agreement with the theoretical prediction.

At home, in 8 Palace Gardens Terrace, Kensington, Maxwell measured the viscosity of gases, with his wife's help, and extended his experiments on colour vision.

In spite of the magnitude and variety of his researches during this period, Maxwell, unlike Faraday, did not curtail his social life. He gave up the ground floor of his house to his brother-in-law, who had a serious illness. The shortage of space made it necessary for him to have his meals in a very small back-room, where he often breakfasted on his knees, because there was no place for another chair. He frequently found time to nurse his brother-in-law.

He did most of his scientific work in the mornings, except when he was entertaining friends. Then he gave up the whole of his days to them, and worked in the night. In the afternoons he rode in the park.

It is said that Maxwell's friendship with Faraday was extended during his King's College days. But good evidence that this personal contact was of profound influence on science is not easily found. Maxwell's acquaintance with Faraday's work seems to have come before his acquaintance with the man, and he seems to have discovered its implications without personal explanation from Faraday. He was not a pupil of Faraday. He worked independently and originally on Faraday's results, and his intellect was mature before he met him.

In addition to his respect for the social duties of his life, he also found much time for the cultivation of "Glenlair." The magnitude of his genius is illustrated by the balance of his life during his most fertile years. His scientific discoveries were not accomplished at the expense of sociability. He had no children, but owing to lack of information it is difficult to say in what degree he sublimated his desire in

research. The circumstance probably affected him deeply, as he was admirably fitted to appreciate children.

In 1866 Maxwell is described "as a man of middle height, with frame strongly knit, and a certain spring and elasticity in his gait; dressed for comfortable ease rather than elegance; a face expressive at once of sagacity and good humour, but overlaid with a deep shade of thoughtfulness; features boldly, but pleasingly marked; eyes dark and glowing, hair and beard perfectly black, and forming a strong contrast to the pallor of his complexion. . . ." He might have been taken for a country gentleman, or northern laird. His strong sense of humour was expressed by a peculiar twinkle of the eyes. He was serenely placid, and genial, and patient when others would have been vexed. In the laboratory he was very neat-handed, and whistled in a half-subdued manner in a sort of running accompaniment to his thoughts, and occasionally would make comments in a soft voice to his dog Tobi.

After he had become professor in Cambridge he once sent a "large amount of calcareous deposit which had accumulated in a curiously oolitic form in a boiler . . . to the Professor of Geology with a request that he would identify the formation. This he did at once, vindicating his science from the aspersion which his brother professor would playfully have cast on it."

Einstein has said that he did not find the discovery of the theory of relativity very difficult. He did not sacrifice his time and interests in a consuming struggle with the problem. He found the solution by normal application and work. Maxwell also seems to have made his discoveries without social sacrifice, or excessive mental strain. When he showed signs of mental strain they seemed to be the secondary effects of physical illness rather than the direct effects of mental exhaustion. Unlike Newton and Faraday he never suffered from mental disorder due to overwork, and his mind preserved its lucidity even when he was dying. He did not lose his full mental vigour until a few months before his death, and he did not realize he was seriously ill until he found he could no longer think creatively. Scientists

such as Maxwell and Einstein, who make great discoveries without very exceptional effort, illustrate clearly the difference between the various degrees of human intelligence. Their great brains have no more difficulty in making great discoveries than little brains in making little discoveries. The great results produced by normal mental efforts show the true magnitude of their intelligences.

In 1861 and 1862 Maxwell published four papers *On Physical Lines of Force.* He had given a mathematical description of Faraday's conception in the first paper written after he had graduated, and which has been discussed. He now began to imagine how the mechanism of the lines of force might work and in the course of this operation discovered the fundamental ideas of the electromagnetic theory of light. In the earliest paper he had "shewn how to deduce the mathematical relations between the electro-tonic state, magnetism, electric currents, and the electro-motive force, using mechanical illustrations to assist the imagination, but not to account for the phenomena."

Now he examined "magnetic phenomena from a mechanical point of view, and to determine what tensions in, or motions of, a medium are capable of producing the mechanical phenomena observed. "He sought by the same hypothesis to "connect the phenomena of magnetic attraction with electro-magnetic phenomena and with those of induced currents."

He starts by searching for a mechanical model of lines of magnetic force. A magnetic force cannot be represented, for example, by a pressure at a point in an ordinary liquid, "because a line of magnetic force has direction and intensity, but has no third quality indicating any difference between the *sides* of the line, which would be analogous to that observed in the case of polarized light." So he supposes "that the phenomena of magnetism depend on the existence of a tension in the direction of the lines of force, combined with a hydrostatic pressure; or in other words, a pressure greater in the equatorial than in the axial direction; the next question is, what mechanical explanation can we give of this inequality of pressures in a fluid or mobile medium?

The explanation which most readily occurs to the mind is that the excess of pressure in the equatorial direction arises from the centrifugal forces of vortices or eddies in the medium having their axes in directions parallel to the lines of force."

Maxwell then examines the properties of space filled with vortices. "We shall suppose at present that all the vortices in any one part of the field are revolving in the same direction about axes nearly parallel, but that in passing from one part of the field to another, the direction of the axes, the velocity of rotation, and the density of the substance of the vortices are subject to change. We shall investigate the resultant mechanical effect upon an element of the medium, and from the mathematical expression of this resultant we shall deduce the physical character of its different component parts." He concludes that the "lines of force indicate the direction of *minimum pressure* at every point of the medium," and the differences of pressure in different directions may be satisfactorily represented by the tendency of the vortices to expand or contract. In his second paper Maxwell inquires "into the physical connexion of these vortices with electric currents," and how they may be set in rotation. He writes: "I have found great difficulty in conceiving of the existence of vortices in a medium, side by side, revolving in the same direction about parallel axes. The contiguous portions of consecutive vortices must be moving in opposite directions; and it is difficult to understand how the motion of one part of the medium can co-exist with, and even produce, an opposite motion of a part in contact with it.

The only conception that has at all aided me in conceiving of this kind of motion is that of the vortices being separated by a layer of particles, revolving each on its own axis in the opposite direction to that of the vortices, so that the contiguous surfaces of the particles and of the vortices have the same motion.

In mechanism, when two wheels are intended to revolve in the same direction, a wheel is placed between them so as to be in gear with both, and this wheel is called an 'idle

wheel.' The hypothesis about the vortices which I have to suggest is that a layer of particles, acting as idle wheels, is interposed between each vortex and the next, so that each vortex has a tendency to make the neighbouring vortices revolve in the same direction with itself."

Maxwell remarks that in epicyclic gears the idle wheels may be capable of translated motion, as in Siemens' governor for steam engines. He starts to consider the mechanics of space filled with vortices separated by a network of idle wheels that can be propelled through space by being carried round the circumference by one rotating vortex and passed on to the next.

The conception of the electro-magnetic ether is now very near, and engineers will be interested to note the help derived by Maxwell from the consideration of epicyclic motions.

After his examination of the mechanics of this model he recapitulates his results. "Magneto-electric phenomena are due to the existence of matter under certain conditions of motion or of pressure in every part of the magnetic field, and not to direct action at a distance between the magnets or currents. The substance producing these effects may be a certain part of ordinary matter, or it may be an ether associated with matter." It must be very rare since no substance except iron "has a large ratio of magnetic capacity to what we call a vacuum."

The lines of magnetic force are along the directions of least pressure, and the inequalities of pressure are due to the centrifugal forces of the vortices. "The effect of these vortices depends on their density, and on their velocity at the circumference, and is independent of their diameter." "The velocity must be very great, in order to produce so powerful effects in so rare a medium." "The size of the vortices is indeterminate, but is probably very small as compared with that of a complete molecule of ordinary matter."

The vortices are separated from each other by a single layer of round particles. The particles "are perfectly free to roll between the vortices and so to change their place,

provided they keep within one *complete molecule* of the substance; but in passing from one molecule to another they experience resistance, and generate irregular motions, which constitute heat. Their motion of translation constitutes an electric current, their rotation serves to transmit the motion of the vortices from one part of the field to another, and the tangential pressures thus called into play constitute electro-motive force."

"The effect of an electric current upon the surrounding medium is to make the vortices in contact with the current revolve so that the parts next to the current move in the same direction as the current. The parts furthest from the current will move in the opposite direction; and if the medium is a conductor of electricity, so that the particles are free to move in any direction, the particles touching the outside of these vortices will be moved in a direction contrary to that of the current, so that there will be an induced current in the opposite direction to the primary one." He explains that when an electric current or a magnet is moved in presence of a conductor, the velocity of rotation of the vortices is altered. "The force by which the proper amount of rotation is transmitted to each vortex, constitutes in this case also an electro-motive force, and, if permitted, will produce currents." "When a conductor is moved in a field of magnetic force, the vortices in it and in its neighbourhood are moved out of their places, and are changed in form. The force arising from these changes consitutes the electro-motive force on a moving conductor."

The idle wheels, or particles between the vortices, were translated from one part of the field to another, when a change in the velocity of rotation of the vortices was being transmitted through the system. Maxwell conceived the stream of particles as an electric current. In this way he visualized Faraday's discovery that currents are produced by changes in the magnetic form, in the vortices. But the model showed how the reverse phenomenon occurred, for if the idle wheels or particles began to be translated or flow through the system, the shapes of the vortices were modified;

a change in the vortices, in the magnetic field, accompanied the flowing particles, the electric current. As J. J. Thomson has explained, when Maxwell came to use his model, "he found that it suggested that *changes* in the electric force would produce magnetic force. The introduction and development of this idea was Maxwell's greatest contribution to Physics. The importance of the step made by Maxwell is indicated by the fact that on the electro-magnetic theory which held the field before his time, electrical waves could not exist, while on his theory all changes in electric and magnetic forces sent waves spreading through space."

In the third part of his paper *On Physical Lines of Force*, Maxwell wrote that "it is necessary to suppose, in order to account for the transmission of rotation from the exterior to the interior parts of each cell, that the substance in the cells possesses elasticity of figure, similar in kind, though different in degree, to that observed in solid bodies. The undulatory theory of light, requires us to admit this kind of elasticity in the luminiferous medium, in order to account for transverse vibrations. We need not then be surprized if the magneto-electric medium possesses the same property."

Maxwell's model effectively illustrated the properties of insulators and conductors. "Bodies which do not permit a current of electricity to flow through them are called insulators. But though electricity does not flow through them, the electrical effects are propagated through them, and the amount of these effects differs according to the nature of the body; so that equally good insulators may act differently as dielectrics."

He explains that when the electric particles are urged in any direction the material of the vortex cells is distorted, and when the urging force is removed the elastic material will return to its former position. Then he considers the relation between the displacement and the force producing it, and deduces from his result the relation between the statical and dynamical measures of electricity, and shows "by a comparison of the electro-magnetic experiments of M. M. Kohlrausch and Weber with the velocity of light as found by M. Fizeau, that the elasticity of the magnetic

medium in air is the same as that of the luminiferous medium, if these two co-existent, co-extensive and equally elastic media are not rather one medium." The agreement between the figures of Kohlrausch and Weber, and Fizeau is so good, "that we can scarcely avoid the inference that *light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena.*"

Maxwell reaches this conclusion in the third part of his paper, which was published in January, 1862. He had now obtained the substance of the equations for describing the electro-magnetic field. He refined and ordered his ideas and composed his memoir on *A Dynamical Theory of the Electro-magnetic Field.* The memoir was read to the Royal Society in 1864. He has dispensed with the model, and describes the properties of the electro-magnetic field by twenty general equations. The electro-magnetic theory of light follows from these equations. He remarks that "the conception of the propagation of transverse magnetic disturbances to the exclusion of normal ones is distinctly set forth by Professor Faraday in his *Thoughts on Ray-vibrations.* The electro-magnetic theory of light, as proposed by him, is the same in substance as that which I have begun to develop in this paper."

The disappearance of the model of the electro-magnetic field from Maxwell's memoir of 1864 is very instructive. It shows that Maxwell was not dominated by the visual concepts of the engineering imagination. After a model had helped him to reach pregnant equations, he discarded it without intellectual discomfort. He explicitly stated that the visual and the mathematical imaginations were of equal value in physical research. "For the sake of persons of different types of mind, scientific truth should be presented in different forms and should be regarded as equally scientific whether it appears in the robust form and vivid colouring of a physical illustration or in the tenuity and paleness of a symbolical expression."

Though Maxwell could use engineering images with at least as much power as any of his contemporaries, he was entirely free from the tendency to assume they were

necessarily more real than mathematical symbols, so he escaped the chief error of the scientific philosophy of the nineteenth century.

Maxwell's model had other fascinating features, besides its suggestions of the electro-magnetic theory of light. His vortices with particles of electricity flowing round them are prophetic of the electron, and the atom with orbital electrons. The electric current was, in fact, a stream of electrons. Yet Maxwell threw these scientific jewels away. When he elaborated his theory in the *Treatise on Electricity and Magnetism*, he did not use the concept of electric particles in order to arrive at his general equations, as he had done when he first discovered them. But when he reaches the discussion of electrolysis he is forced to speak of a *molecule of electricity*. He writes: "This phrase, gross as it is, and out of harmony with the rest of this treatise, will enable us at least to state clearly what is known about electrolysis, and to appreciate the outstanding difficulties."

If Maxwell had been an exclusively visual thinker he could probably not have discarded the idea of particles of electricity. But he did discard the idea, though he had made his greatest discovery with its help, and like Faraday, had been forced to it by the facts of electrolysis. He was probably influenced by Faraday's shyness of the idea of atoms of electricity, besides the working of his own mind. Thus Maxwell's prejudice against the particulate conception of electricity was probably acquired through Faraday from Davy, who had transmitted the prejudice to his pupil. Even at the moment when he deserted the particulate idea he was not without inspiration. Reflexion on the modern wave-theory of particles shows how suitably the word "gross" describes the idea of an electron as a simple particle. Maxwell rejected the particle also because he felt that the idea was not sufficiently subtle.

After the five strenuous years in London, Maxwell resigned from his chair at King's College in 1865, and retired to "Glenlair." He was always more attached to his little estate than to professional position. In this he was loyal to the traditions of his social class. In 1868, at the

early age of thirty-seven, he was invited to become the Principal of the United College in the University of St. Andrews, in effect, the Principal of the University, but even this could not tempt him from "Glenlair." He spent his days writing the great *Treatise on Electricity and Magnetism*, and enjoying the social and religious duties of a minor laird. A special post-box was erected near his house for his large correspondence. He had been deeply interested in the endowment of the local church and had made large contributions to it. His attendance at the services was very regular, and he conducted the family prayers in his own household, frequently composing extempore prayers. After he had returned from the church services on Sundays he frequently read works of the old divines. Maxwell's religious interests are of great psychological importance, but unfortunately inadequate accounts of them have survived, apparently owing to the mistaken idea of his contemporary relatives and friends, that the world was not entitled to know them. His biographer, Lewis Campbell, was a priest, and writes that he would not describe things "wherewith the stranger intermeddles not." While we are deeply grateful for Campbell's biography, it is impossible not to wish that he could himself have avoided intermeddling with the biographical data of a world-genius. He gives excerpts from several religious letters written by Maxwell to his wife. One is dated June 28th, 1864.

"I can always have you with me in my mind—why should we not have our Lord always before us in our minds, for we have His life and character and mind far more clearly described than we can know anyone here? If we had seen Him in the flesh we should not have known Him any better, perhaps not so well. Pray to Him for a constant sight of Him, for He is man that we may be able to look to Him, and God, so that He can create us anew in His own image."

Some features in this passage are closely connected with the processes of Maxwell's scientific imagination. The General Equations of the Electro-magnetic Field were more real to him than material phenomena he could know in the

laboratory. Physicists have often wondered why Maxwell made no attempt to prove experimentally the existence of electro-magnetic waves. He probably felt he was better acquainted with the waves through the medium of the General Equations, and would "not have known them any better, perhaps not so well," if he had met them in the laboratory. As the literary description of our Lord in the New Testament provides a clearer knowledge of his character than personal acquaintance can provide of the character of a friend, so the literary description of electro-magnetic waves in the General Equations provides a clearer knowledge of the properties of these waves than an experimental acquaintance with them.

During his engagement Maxwell studied the Bible with his future wife. In a letter dated May 13th, 1858, he writes:

"I have been reading again with you, Eph. vi. Here is more about family relations. There are things which have meanings so deep that if we follow on to know them we shall be led into great mysteries of divinity. If we despise these relations of marriage, of parents and children, of master and servant, everything will go wrong, and there will be confusion as bad as in Lear's case. But if we reverence them, we shall even see beyond their first aspect a spiritual meaning. For God speaks to us more plainly in these bonds of our life than in anything that we can understand. So we find a great deal of Divine Truth is spoken of in the Bible with reference to these three relations and others."

Here Maxwell accepts material relationships with the belief that acquaintance with them will lead to spiritual understanding. This is the reverse of the other process of believing in the superiority of literary and mathematical guides to material truths. He proceeds from the contemplation of material relationships to spiritual truths, from the model of the electro-magnetic field to the equations.

Maxwell reinforces his theory of society from the Bible. The positions of master and servant are sanctioned by Scripture. As his life at "Glenlair" shows, he was feudalist,

with the sense of social responsibility felt by true feudal leaders.

The influence of the New Testament is seen also in his interpretation of self-sacrifice. During the last years of his life his wife was an invalid. He nursed her personally with the most assiduous care. At one period he did not sleep in a bed for three weeks, though he delivered his lectures and superintended the laboratory as usual. When the earlier symptoms of his own fatal disease became evident to himself he told no one of them for a long time. As he grew worse and suffered severe pain he never complained, except that he would not be able to continue to nurse his sick wife.

Many must regret that Maxwell did not discover more modern sources of inspiration for his thought on social and ethical problems. They cannot be satisfied with his rôle of gentleman-farmer or laird in the middle of the nineteenth century. The modernity of Maxwell's science, and the antiquity of his sociology and religion appear incongruous. But it may be noted that though his views on sociology and religion were antique, they were superior to those of nearly all his scientific contemporaries. He at least thought about these problems, and if he was unable to find modern answers to them, he learned enough of them to avoid the intellectual philistinism of his time. Like William Morris, he escaped, though in the slightly ridiculous disguise of a mediæval uniform. If Maxwell had arrived simultaneously at modern views on science, sociology and religion, and had expressed them, he would never have become the parent of the physical thought of the twentieth century. His antique views on the relation between master and man, and on family prayers served as an effective camouflage to his revolutionary work in Cambridge. Without his mediæval habiliments he would never have secured a commanding position in that University, and in the development of capitalist culture.

During his retirement at "Glenlair," Maxwell occasionally visited Cambridge to act as examiner. He examined in the Mathematical Tripos in 1866, 1867, 1869, 1870. He

started a very important reform of this examination by introducing problems on Electricity and Heat. He skilfully engaged the support of William Thomson in this movement. Its significance lay in the adaptation of Cambridge scientific training to the needs of a new form of society. Since Newton, Cambridge had been the chief school for the ablest English scientists. The governing class looked to Cambridge for the sort of scientific knowledge it required. Before the nineteenth century its chief requirement was advanced astronomy, owing to the dependence of accurate navigation on this science, and the dependence of a maritime power on navigation. When the astronomical tradition had become established it persisted after its aim had been adequately accomplished. The nineteenth-century industrialists required the science of heat and electricity in order to develop the steam-engine and dynamo. Maxwell was the leader in adaptation of physical science at Cambridge to these needs.

The movement for the extension of science teaching at Cambridge led to the appointment of a committee, which reported in 1869 that the science teaching required strengthening by the foundation of a professorship and demonstratorship of physics, and a university laboratory. The cost of the laboratory was estimated at £6,300. The University was not immediately able to find so much money for this object, but in 1871 the University chancellor, the seventh Duke of Devonshire, offered personally to provide the money, so the University proceeded to appoint a professor. William Thomson was approached first, but did not wish to leave Glasgow. He suggested Hopkinson would be a suitable candidate. Then Helmholtz was approached, but he had just been appointed director of a new laboratory in Berlin. The authorities now attempted to persuade Maxwell to accept it. After much effort, they were successful. They had the wonderful fortune of attracting the greatest of the three geniuses.

In spite of being a duke, the Chancellor had been second Wrangler and first Smith's Prizeman in his youth. The family name of the Dukes of Devonshire was Cavendish,

and the famous Henry Cavendish was a relative of his ancestors. Henry Cavendish left an immense fortune, but did not bequeath any of it to scientific research. The Duke's own interest, and these circumstances, probably suggested his important offer. It is interesting to note that he regretted his own academic career and ordered his son's otherwise, as he considered his own application to science had unfitted him to exercise the political influence pertaining to his social position.

After his appointment, Maxwell's first task was the design and construction of the laboratory. He undertook the work with great energy and enthusiasm, and carefully planned schemes of laboratory arrangement, teaching and research. The laboratory proved to be considerably more expensive than the first estimate, but the Duke agreed to meet the whole of the cost. It was completed and opened in 1874. Many readers may be astonished that the University of Cambridge had no laboratory for the experimental study of physical science before 1874, and that the famous Cavendish Laboratory is only sixty years old. Before that date, physical experiments were made in private rooms, with simple and often crude apparatus. Newton and Stokes had made fundamental experimental discoveries under such conditions. But the development of precision in industry created the demand for exact measurement, besides an understanding of principle. Exact measurement requires high skill and refined apparatus, for which intelligence without skill is a partial substitute only.

Much of the credit for Maxwell's enthusiasm and practical judgment in designing the Cavendish Laboratory is due to his father. John Maxwell had trained his son to be interested in mechanics, and the design and management of buildings. He had done much to free him from the ancient prejudice against manual activity deeply incorporated in Graeco-Roman culture. The mediæval universities had perpetuated this attitude, which still persisted strongly in Oxford and Cambridge, and has not yet disappeared. It is an exceptionally clear example of the expression of class struggle in the realm of culture. The creation of the

prejudice against the cultural prestige of manual activity, to which Plato made a large contribution, was semi-consciously designed to discredit the social status of manual work, in order that power might be retained by military, religious and other classes of persons that did not work with their hands. This prejudice has complicated historical roots and is of great social significance. The official recognition of the cultural status of experimental physics at Cambridge had to be obtained in opposition to it. Social force and strategic intelligence were necessary to overcome this opposition, and the force came from the industrialists and the strategy from Maxwell. The strategic sense is implicit in much of Maxwell's work, and enabled him to capture results with apparently inadequate mathematical forces. Sometimes he explicitly writes on strategy, as in his paper on *The Dynamical Evidence of the Molecular Constitution of Bodies*. He writes: "It is only a small part of the theory of the constitution of bodies which has as yet been reduced to the form of accurate deductions from known facts. To conduct the operations of science in a perfectly legitimate manner, by means of methodized experiment and strict demonstration, requires a strategic skill which we must not look for, even among those to whom science is most indebted for original observations and fertile suggestions. It does not detract from the merit of the pioneers of science that their advances, being made on unknown ground, are often cut off, for a time, from that system of communications with an established base of operations which is the only security for any permanent extension of science."

One of Maxwell's ancestors claimed to have provided Rodney with the tactics that won the battle of Dominique. Maxwell won a still greater battle, the battle of experimental physics at Cambridge. Rodney fought for the mercantilists, while Maxwell was a cultural general of the industrialists.

John Maxwell's contribution to culture was the preservation of the communications between his son and the industrialist base whose needs inspired the demand for experimental physics.

With his usual generosity Maxwell bought much apparatus for the laboratory at his own expense and presented his personal apparatus. After his death his wife gave his library of scientific books, and on her death she bequeathed about £6000 for the foundation of a studentship to be held at the laboratory.

Maxwell delivered his introductory lecture on experimental physics as Cavendish Professor in 1871. The entertaining circumstances concerning the delivery of his lecture have been described at the beginning of this chapter. He refers to the future laboratory as the Devonshire Physical Laboratory. In the first paragraph he writes: "The University of Cambridge, in accordance with that law of its evolution by which, while maintaining the strictest continuity between the successive phases of its history, it adapts itself with more or less promptness to the requirements of the times, has lately instituted a course of experimental physics. This course of study, while it requires us to maintain in action all those powers of attention and analysis which have been so long cultivated in the University, calls on us to exercise our senses in observation and our hands in manipulation. The familiar apparatus of pen, ink and paper will no longer be sufficient for us, and we shall require more room than that afforded by a seat at a desk and a wider area than that of the blackboard."

Then he discusses how to "vitalize this new organ." One of its first activities should be the creation of a spirit of sound criticism, as "we are daily receiving fresh proofs that the popularization of scientific doctrines is producing as great an alteration in the mental state of society as the material applications of science are effecting in its outward life." The cultivation of "sound dynamical ideas has already effected a great change in the language and thoughts even of those who make no pretensions to science." He fears that the public may be converted to the most absurd opinions if "expressed in language, the sound of which recalls some well-known scientific phrase." He refers again to the critical functions of the workers in the laboratory and says: "Our principal work, however, in the laboratory

must be to acquaint ourselves with all kinds of scientific methods, to compare them, and to estimate their value. It will, I think, be a result worthy of our University, and more likely to be accomplished here than in any private laboratory if by the free and full discussion of the relative value of different scientific procedures we succeed in forming a school of scientific criticism and in assisting the development of the doctrine of method."

He deprecates the suggestion that the progress of physical science is at an end, and that physicists will not be able to do more than refine their "measurements to another place of decimals." If this state of things is approaching, the laboratory could have no place in the University and would have to be classed with other "great workshops of our country, where equal ability is directed to more useful ends. But we have no right to think thus of the unsearchable riches of creation."

He explains that the history of science does not sanction the view that discovery can be at an end.

Then he sketches the nature and desirability of teamwork in research. He describes how Bacon's notion of "Experiments in Concert" was realized by Humboldt, Gauss, Weber and Leyser, the instrument-maker, in their organization of the Magnetic Union, and how "the scattered forces of science were converted into a regular army and emulation and jealousy became out of place, for the results obtained by any one observer were of no value till they were combined with those of others." He says "we hope one day to perform" such experiments "in our laboratory."

It is not quite clear from Maxwell's description whether he envisaged concerted experiments within the laboratory or that the laboratory should have a place in concerted international researches; but his direction of attention to the general idea of concerted research has had an important historical influence on the Cavendish Laboratory. The sociologist will see in this conscious arrangement of concerted research the penetration of the idea of subdivision of labour and the methods of factory production into physical

research. Maxwell refers in fact to "the other great workshops of our country."

He explains that knowledge which comes through the combined apprehension of its mathematical and its experimental aspects is "of a more solid, available and enduring kind than that possessed by the mere mathematician or the mere experimenter." New ideas can arise only by "wrenching the mind away from the symbols to the objects and from the objects back to the symbols. This is the price we have to pay." He admits the amount of a man's mental energy is limited, but its most effective use is to be obtained by a proper distribution between mathematical study and experiment, and not in an entire concentration on the mathematics. "A great part of our fatigue often arises, not from those mental efforts by which we obtain the mastery of the subject, but from those which are spent in recalling our wandering thoughts." Maxwell seems to suggest that skilful control of the attention should leave energy for experimental work, and perhaps that experimental work may prove a wholesome means of distraction from mathematics and reinforcement of the power of concentration. Maxwell writes that "there may be some mathematicians who pursue their studies entirely for their own sake. Most men, however, think that the chief use of mathematics is found in the interpretation of nature. . . . I have known men who, when they were at school, never could see the good of mathematics, but who, when in after life they made this discovery, not only became eminent as scientific engineers but made considerable progress in the study of abstract mathematics. If our experimental course should help any of you to see the good of mathematics it will relieve us of much anxiety, for it will not only ensure the success of your future studies but it will make it much less likely that they will prove injurious to your health."

"It is very necessary that those who are trying to learn from books the facts of physical science should be enabled by the help of a few illustrative experiments to recognize these facts when they meet with them out of doors. Science

appears to us with a very different aspect after we have found out that it is not in lecture-rooms only, and by means of the electric light projected on a screen, that we may witness physical phenomena, but that we may find illustrations of the highest doctrines of science in games[1] and gymnastics, in travelling by land and by water, in storms of the air and of the sea, and wherever there is matter in motion. This habit of recognizing principles amid the endless variety of their action can never degrade our sense of the sublimity of nature or mar our enjoyment of its beauty. On the contrary, it tends to rescue our scientific ideas from that vague condition in which we too often leave them buried among the other products of a lazy credulity, and to raise them into their proper position among the doctrines in which our faith is so assured, that we are ready at all times to act on them."

He deplores the growth of "a narrow professional spirit" among scientists, and explains that it is their duty to preserve an acquaintance with literary and historical studies. Not long ago scientists were regarded as misanthropes whose devotion to abstractions has made them "Insensible alike to the attractions of pleasure and to the claims of duty. In the present day men of science are not looked upon with the same awe or with the same suspicion. They are supposed to be in league with the material spirit of the age and to form a kind of advanced Radical party among men of learning."

In the last part of his lecture he says his first course will be on Heat. He remarks that the motion of the Dissipation of Energy cannot be understood by Thermodynamics alone, but requires some definite theory of the constitution of bodies.

"Two theories of the constitution of bodies have struggled for victory with various fortunes since the earliest ages of speculation: one is the theory of the universal plenum, the other is that of atoms and void.

The theory of the plenum is associated with the doctrine

[1] For a discussion of the influence of games on physics, see *Osiris and the Atom* by J. G. Crowther.

of mathematical continuity, and its mathematical methods are those of the Differential Calculus, which is the appropriate expression of the relations of continuous quantity.

The theory of atoms and void leads us to attach more importance to the doctrines of integral numbers and definite proportions; but in applying dynamical principles to the motion of immense numbers of atoms, the limitation of our faculties forces us to abandon the attempt to express the exact history of each atom, and to be content with estimating the average condition of a group of atoms large enough to be visible. This method of dealing with groups of atoms, which I may call the statistical method, and which in the present state of our knowledge is the only available method of studying real bodies, involves an abandonment of strict dynamical principles and an adoption of the mathematical methods belonging to the theory of probability. It is probable that important results will be obtained by the application of this method, which is as yet little known and is not familiar to our minds. If the actual history of Science had been different, and if the scientific doctrines most familiar to us had been those which must be expressed in this way, it is possible that we might have considered the existence of a certain kind of contingency a self-evident truth and treated the doctrine of philosophical necessity as a mere sophism."

So Maxwell closes his lecture with a prevision of the ideas of the quantum theory and the principle of uncertainty, or indeterminacy as described by some.

His introductory lecture is one of the great documents in the history of science. It is the code that he composed for the policy of the Cavendish Laboratory. As everyone knows, it has been followed with results without parallel in the history of human institutions. No human institution has had four successive directors of the quality of Maxwell, Rayleigh, Thomson and Rutherford, or sixty years of continued activity at the highest level of genius. J. J. Thomson has written that Maxwell "left us the proud heritage of a great name. How great that heritage is was not realized until long after his death."

Maxwell lived for eight years after his acceptance of the Cavendish chair. Besides the creation of the Laboratory, he devoted much time to the pious work of editing *The Electrical Researches of the Honourable Henry Cavendish.* Some have regretted that he gave so much time to this work, but it is clear that breadth of culture was an essential part of his genius and historical studies were necessary for its nourishment. "Some distributions of energy we know are more useful than others." After so many examples of prescience it is permissible to suppose that Maxwell knew what was best for the future of science.

During his later years Maxwell became less reserved from the public. He was the president of the mathematics and physics section of the British Association in 1870, and delivered an address on the relation of Mathematics and Physics. It contains the well-known passage on that "hidden and dimmer region where Thought weds Fact, where the mental operation of the mathematician and the physical action of the molecules are seen in their true relation. Does not the way to it pass through the very den of the metaphysician, strewed with the remains of former explorers and abhorred by every man of science?"

His *Discourse on Molecules* at the Bradford meeting in 1873 contained some clear statements disconcerting to those who accepted superficial theories of evolution. He said that "no theory of evolution can be formed to account for the similarity of molecules, for evolution necessarily implies continuous change, and the molecule is incapable of growth or decay, of generation or destruction." The modern writer would substitute the constancy of the electronic charge for that of the molecule. He continues: "the exact quality of each molecule to all molecules of the same kind gives it, as Sir John Herschel has well said, the essential character of a manufactured article, and precludes the idea of its being eternal and self-existent. . . . We have reached the utmost limits of our thinking faculties when we have admitted that because matter cannot be eternal and self-existent it must have been created."

Maxwell had no sympathy with the superficial materialists bred in the first wave of evolutionary enthusiasm. In 1874 Tyndall was president of the British Association meeting at Belfast. His address drew a copious flow of verse from Maxwell whose muse was exceptionally active in that year. In his *Notes of the President's Address* he writes:

In the very beginning of science,
   the parsons, who managed things then,
Being handy with hammer and chisel, made
   gods in the likeness of men;
Till Commerce arose, and at length
   some men of exceptional power
Supplanted both demons and gods by
   the atoms, which last to this hour.

From nothing comes nothing, they told
   us, nought happens by chance but by fate;
There is nothing but atoms and void,
   all else is mere whims out of date!
Then why should a man curry favour
   with beings who cannot exist,
To compass some petty promotion in
   nebulous kingdoms of mist?

Thus the pure elementary atom, the
   unit of mass and of thought,
By force of mere juxtaposition to life
   and sensation is brought;
So, down through untold generations,
   transmission of structureless germs
Enables our race to inherit the thoughts
   of beasts, fishes and worms.
We honour our fathers and mothers,
   grandfathers and grandmothers too;
But how shall we honour the vista
   of ancestors now in our view?
First, then, let us honour the atom,
   so lively, so wise, and so small;
The atomists next let us praise, Epicurius,
   Lucretius, and all;
Let us damn with faint praise Bishop
   Butler, in whom many atoms combined

To form that remarkable structure, it
pleased him to call—his mind.
Last, praise me the noble body to which,
for the time, we belong,
Ere yet the swift whirl of the atoms has
hurried us, ruthless, along,
The British Association—like Leviathan
worshipped by Hobbes,
The incarnation of wisdom, built up
of our witless nobs,
Which will carry on endless discussions,
when I, and probably you,
Have melted in infinite azure—
in English, till all is blue.

His friends, Tait and Balfour Stewart, published a book named *Paradoxical Philosophy.* Maxwell wrote an ode to the hero of this work, "Hermann Stoffkraft, Ph.D.," which contains the stanza:

But when thy Science lifts her pinions
In Speculation's wild dominions,
We treasure every dictum thou emittest,
While down the stream of Evolution
We drift, expecting no solution
But that of the survival of the fittest.
Till, in the twilight of the gods,
When earth and sun are frozen clods,
When, all its energy degraded,
Matter to æther shall have faded;
We, that is, all the work we've done,
As waves in æther, shall for ever run
In ever-widening spheres through heavens beyond the sun.

As J. J. Thomson has remarked, this passage is remarkably suggestive of recent theories on the expansion of the universe and the destiny of matter as transformed into waves that travel through the universe for ever.

After Maxwell returned to Cambridge as professor some of the members of the "Apostles" club of his undergraduate period again met for the discussion of philosophical questions. The essays of his mature years illustrate the width of his culture. In his essay on *Science and Free Will* he

explains that in astronomy past and future may be deduced by changing the sign of the time symbol, but in the diffusion of matter and heat, which depend on statistical mechanics, "the prophetical problem is always capable of solution; but the historical one, except in singular cases, is insoluble. There may be other cases in which the past, but not the future, may be deducible from the present."

Heisenberg has recently pointed out that his principle of uncertainty implies that the past may be completely deduced from the present, but the future cannot.

In 1878 he wrote a paper on *Psychophysik*. After examining the various views on psycho-physical parallelism he writes:

"In this search of information about myself from eminent thinkers of different types I seem to have learnt one lesson, that all science and philosophy and every form of human speech is about objects capable of being perceived by the speaker and the hearer; and that when our thought pretends to deal with the Subject, it is really only dealing with an Object under a false name. The only proposition about the Subject, namely, 'I am,' cannot be used in the same sense by any two of us, and therefore it can never become science at all."

Readers will see in this passage the axiom of what L. Hogben has named the *Publicist* philosophy.

Pure mathematicians have recently become interested in the researches of Maxwell and Tait in topology. This is the science of positional analysis or *analysis situs*, in which the ideas of proximity and linkage are more important than those of size and shape. Leibnitz foresaw the importance of topology, but he was unable to make any contribution owing to the difficulty of its processes. The first contributions came from Euler and Gauss. The properties of linked bodies and surfaces, such as knots in ropes, depend on topological principles. Tait was interested in the properties of knots, and Maxwell was concerned with linked surfaces that arose in his theory of electro-magnetism. The development of topology, which is one of the features

of contemporary mathematics, was started by Poincaré, who published some of his most important papers in London, in memory, it is said, of the topological researches of Maxwell and Tait.

After some dyspeptic symptoms that he had ignored, Maxwell became seriously ill in 1879. He learned that his life must end swiftly, and he died on November 5th, aged forty-eight. Before concluding this chapter it may be recalled that Maxwell's prophecy of the existence of electro-magnetic waves was proved by the experiments of Hertz in 1887. His certainty of their existence seemed to have removed the impulse to search experimentally for them himself, and, after all, those waves that are the foundation of radio communication were but one of the numerous discoveries made by his genius. The significance of radio communication for humanity is not greater than the creation and legislation of the Cavendish Laboratory; or of statistical mechanics, the parent of the quantum theory; or of the equations of the electro-magnetic field that led to the theory of relativity; or of Maxwell's example as a man of science and culture: as a civilized man.

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